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Vol. 132, No. 3424

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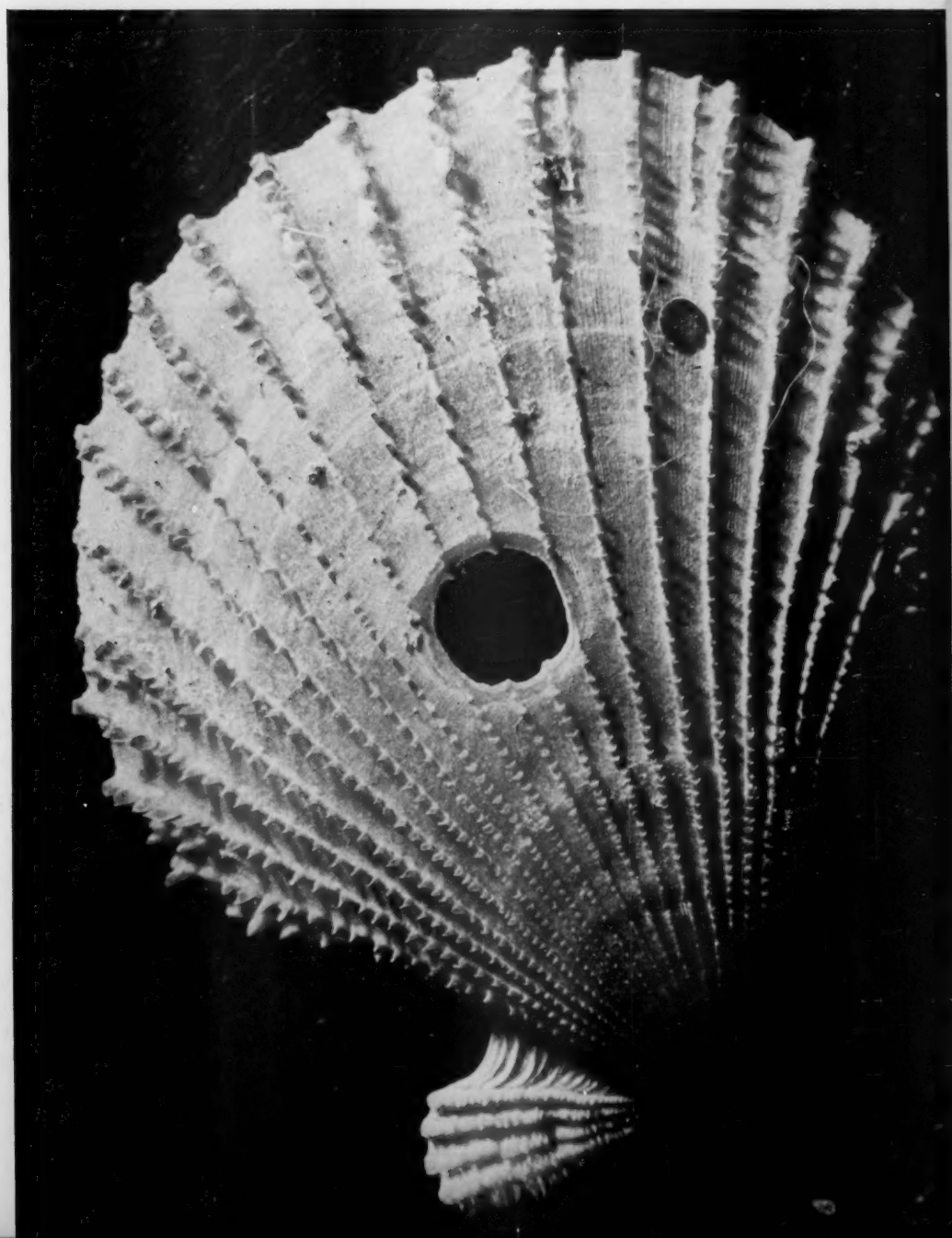

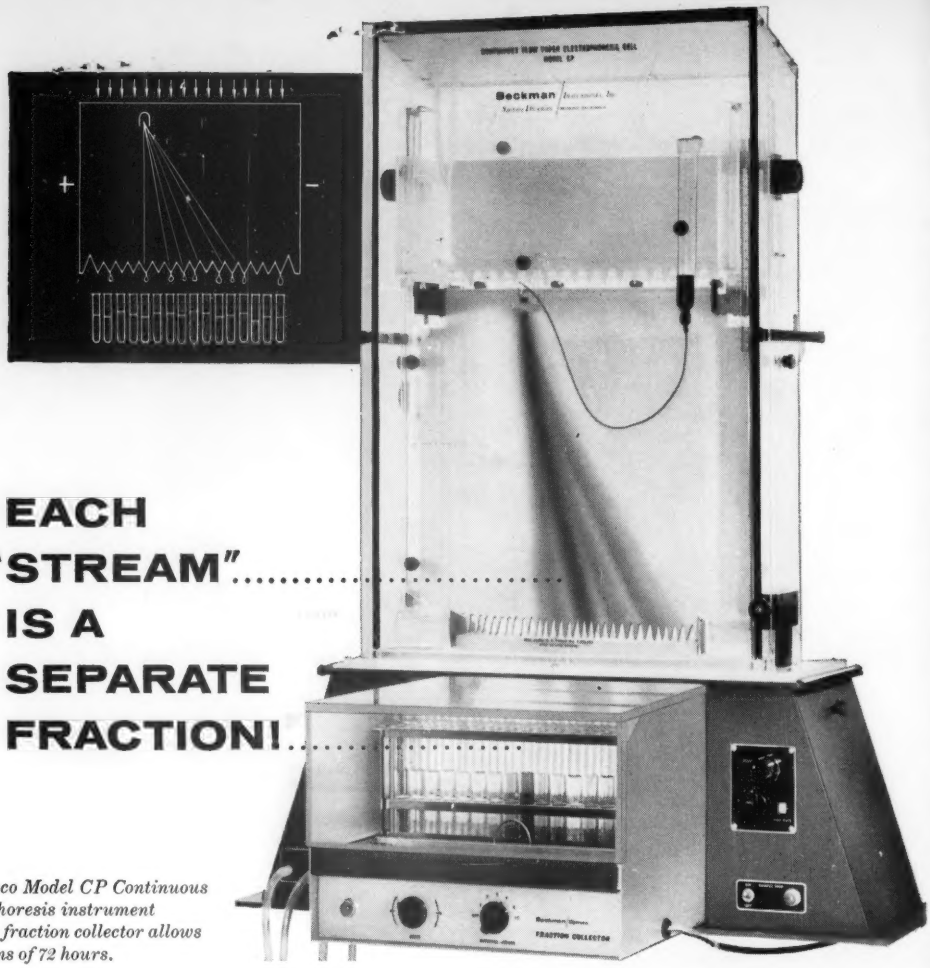

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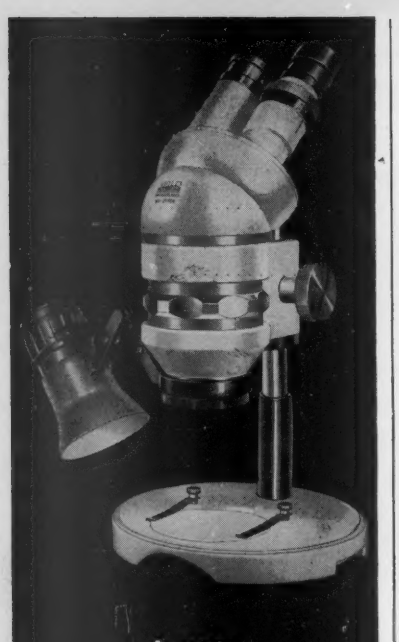
Shell of a pictinid bivalve through which a gastropod has drilled a hole 3 millimeters in diameter. The shell was collected by the Danish *Galathea* Expedition 1950–52 in the Australian Bight at a depth of 440 meters, and was made available by the Zoological Museum, Copenhagen. The photograph was made by S. M. Boone and M. R. Carriker. [M. R. Carriker, University of North Carolina]



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Letters

Quantum Phenomena in Biology

C. Reid (*1*) discusses (among other topics) some recent trends in research on the visual process. In so doing, he keeps the action of rod and cone cells separate, referring to the former as a "monochromatic mechanism" and to the latter as a "trichromatic color-vision process." Wald (*2*) presented a very interesting set of deductions and speculations on the action of rhodopsin and the effect of its bleaching on rod *and* cone activity. The hypothesized structure of both rods and cones is one of compartments containing rhodopsin molecules. The number of compartments per cell can vary, as can the number of rhodopsin molecules per compartment. Sensitivity of a cell is a direct function of the number of compartments and of the amount of rhodopsin per compartment: the more rhodopsin, the higher the probability that that cell will absorb a quantum of light. Several deductions follow from a statistical theory based upon Wald's suggestions—for example, light and dark adaptation phenomena and scotopic and photopic luminosity curves. One new prediction was derived and tested with critical flicker fusion (*3*). The data showed a closer approximation to Wald's conceptualizations than to Crozier's (*4*).

With monochromatic light sources, similar predictions would be difficult to make. Again, some well-grounded speculations may be cited (*5*). First, it is interesting to note that Reid talks of a "trichromatic color-vision process in the cone cells" and observes that "none of the three mammalian cone-cell pigments (these have not yet been isolated) is rhodopsin." Young and Helmholtz started the trichromatic theories; Hering and Ladd-Franklin opposed them; Granit and Hartridge presented ample evidence that perhaps 7- or 11-color theories may be more correct (see *6*).

Trichromatic theories are still in vogue today, due to the existence of three physical primaries, or to our language habits, or to inertia, or to some combination of these, despite the evidence. From the work of Granit and Hartridge it is only one more step to a statistical theory of color vision. This is the step Shaw has taken. One could return to Wald from Shaw's theory and conceptualize compartments containing varying amounts of chlorophyll, xanthophyll, carotene, and other optically active substances. These varying amounts would be distributed normally (like rhodopsin) among cones and cone compartments, so that any given cone might contain close to the mean value of chlorophyll; or it might contain an amount of chlorophyll three standard deviations above the mean and would therefore be a "green cone"; or it might contain a large amount of xanthophyll and be a "yellow cone." Such a theory would seem to be more promising and closer to the content of Reid's paper than a trichromatic theory. I leave the details of such a theory to someone better versed in photochemistry than I.

HOWARD F. GALLUP
Psychology Department, Lafayette College, Easton, Pennsylvania

References and Notes

1. C. Reid, *Science* **131**, 1078 (1960).
2. G. Wald, *ibid.* **119**, 887 (1954).
3. H. F. Gallup, *NAMC-ACEL-341* (16 Aug. 1957).
4. W. J. Crozier, *Proc. Natl. Acad. Sci. U.S.* **28**, 65 (1940).
5. W. A. Shaw, *Psychol. Rev.* **63**, 228 (1956).
6. S. H. Bartley, "The psychophysiology of vision," in S. S. Stevens, *Handbook of Experimental Psychology* (Wiley, New York, 1951), chap. 24.

I believe that Gallup's interesting ideas about cone vision cannot be reconciled with the experimental facts. For instance, Rushton [*Proc. Natl. Acad. Sci. U.S.* **45**, 114 (1959)] has shown, by examination of the reflection spectrum of the fovea centralis of red-blind (protanopic) individuals, the absence of a pigment throughout the red region of the spectrum. Because of the widespread occurrence of this phenomenon, it seems very unlikely that more than one pigment is missing, and consequently this single pigment must be effective over a considerable region of the spectrum. A second pigment, effective in the blue-green region, was also identified by Rushton, and it seems quite reasonable to conclude that a third must exist, covering the blue part of the spectrum. There is nothing in Rushton's spectra to indicate the presence of rhodopsin in the cone cells.

C. REID
University of British Columbia, Vancouver, Canada

Pseudo Science and Censorship

With reference to the editorial "Rebutting the preposterous" [*Science* **131**, 1163 (22 Apr. 1960)], two important questions are implied therein.

1) Who is to judge that grey area, between "accepted science" and pseudo or crackpot science? For it is here that new discoveries arise, and here originate the "breakthroughs" that all are so eagerly seeking. I remember well when the expanding universe and virus as a causative agent in cancer were two ideas believed to have very little merit —then pseudoscientific, if you will.

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2) Who shall constitute the "Committee on Censorship by Selection," responsible to see that the general public shall be permitted to read only "acceptable" popularized scientific reporting?

The attitude of many scientists and the hubbub of the writer-scientists who forced the suspension of publication of a "pseudoscientific" book are reminders of another era. But the shoe is on the other foot. Remember the Condon case and the committees which judged "guilt by association?" Is an individual to be judged a pseudoscientist by reason of his association with ideas which are not acceptable to the "Committee on Censorship by Selection?" The cost of complacent conformity comes high, especially just now.

H. C. DUDLEY

46 Colonial Avenue,
Garden City, New York

Closer Scientific Contact with Czechoslovakia

For a period of 10 years there has been little scientific contact between the Western democracies and Czechoslovakia. During a recent visit to that country, at the invitation of the Czechoslovakian Academy of Sciences, I had an opportunity to meet many scientists, especially botanists and agriculturists, and enjoyed a most cordial and delightful reception. As almost everywhere in the world, support of scientific work has greatly increased, and a large number of serious and dedicated young men are devoting their lives to science.

In the Austro-Hungarian empire there was only one university in the territory which is now Czechoslovakia, the Charles University in Prague, with two evenly supported divisions, one Czech and one German. This equality in support went so far that two identical buildings for botany were erected in the botanical garden, one for Professor Molisch and the other for Professor Nemeč. Now one of the buildings is the laboratory of plant physiology and genetics; the other is for plant taxonomy. Several other universities and colleges were created in the Czechoslovakian Republic before World War II, like the Masaryk University and the Agricultural College in Brno. These institutions are thriving, with a large enrollment and adequate financial support for research and teaching.

In addition to the educational institutions, a number of research institutions have been created during the last 10 years. They are now administered under three Academies: the Czechoslovakian and the Slovakian Academies of Science and the Czechoslovakian Academy of Agriculture.

These academies are administered by the members and are operated as departments of the government, with their own appropriations. When in 1950–51 the first research institutes for fundamental investigations were organized, there was no government agency under which they could properly be set up, and the existing academies were reorganized around their original membership to administer the research institutes.

The greater part of my visit was spent in Prague, especially at the Institute of Biology of the Czechoslovakian Academy of Sciences. This institute consists of a number of special sections; I was most interested in Plant Physiology and Physiological Genetics, of which Dr. Blatzky is director. There are approximately a dozen research workers in this group, studying problems ranging from photosynthesis, plant hormones, and sterile plant culture to the biochemistry of plant development. The laboratories are well-equipped, with apparatus largely constructed in Czechoslovakia and apparently of excellent construction. Only approximately 20 percent of the research apparatus had come from England or America. It was interesting to note that the prices of scientific apparatus were just about the same there as in America.

Personal contact with the research workers was very pleasant, and everyone endeavored to speak English, although German and Russian are the foreign languages most generally spoken by the Czechoslovakian research workers. I greatly appreciated their courtesy in speaking English.

It is a rewarding experience for any scientist to visit Czechoslovakia. Arrangements for trips are most easily handled through the Foreign Department of the Czechoslovakian Academy of Sciences in Prague. I also hope that it will be possible for Czechoslovakian scientists to come and work for some time in the United States.

It is remarkable how much has been accomplished during the last 10 years in the field of science in Czechoslovakia, particularly after the complete suspension of all teaching and most research during the war years, when the universities were closed. In this resurgence of science, Russian and Polish scientists have played an important role, inasmuch as Czechoslovakians received most of their foreign training in those countries.

There are some interesting new developments in Czechoslovakian science. Since no young, trained people were available for research at the end of the war, a new system of apprenticeship was inaugurated in the institutions. These positions could be filled by young persons who wished to become scientists but who had no formal training in research at the universities. These persons are employed for a 3-year period, working with qualified research scientists and thus becoming acquainted with various research techniques. After this 3-year apprenticeship they receive a diploma and can continue working at the universities. In this way students can acquire research experience without overburdening the universities.

Botanical research problems under investigation at the universities and research institutes cover the same range as in the United States, with probably somewhat less emphasis on genetics and more on ecology. One interesting development was that several botanists were engaged in producing film sequences on their research work to be shown as "shorts" in the State movie theaters. They were surprised to learn that such educational films are not shown in the United States. But they never had seen any of Disney's natural history films. It would be an excellent idea if such films could be shown, to acquaint Czechoslovakians with another side of our cultural accomplishments and to show that people here in the United States are also concerned with problems other than atomic bombs and rockets.

F. W. WENT

Missouri Botanical Garden,
St. Louis, Missouri

The Space Race

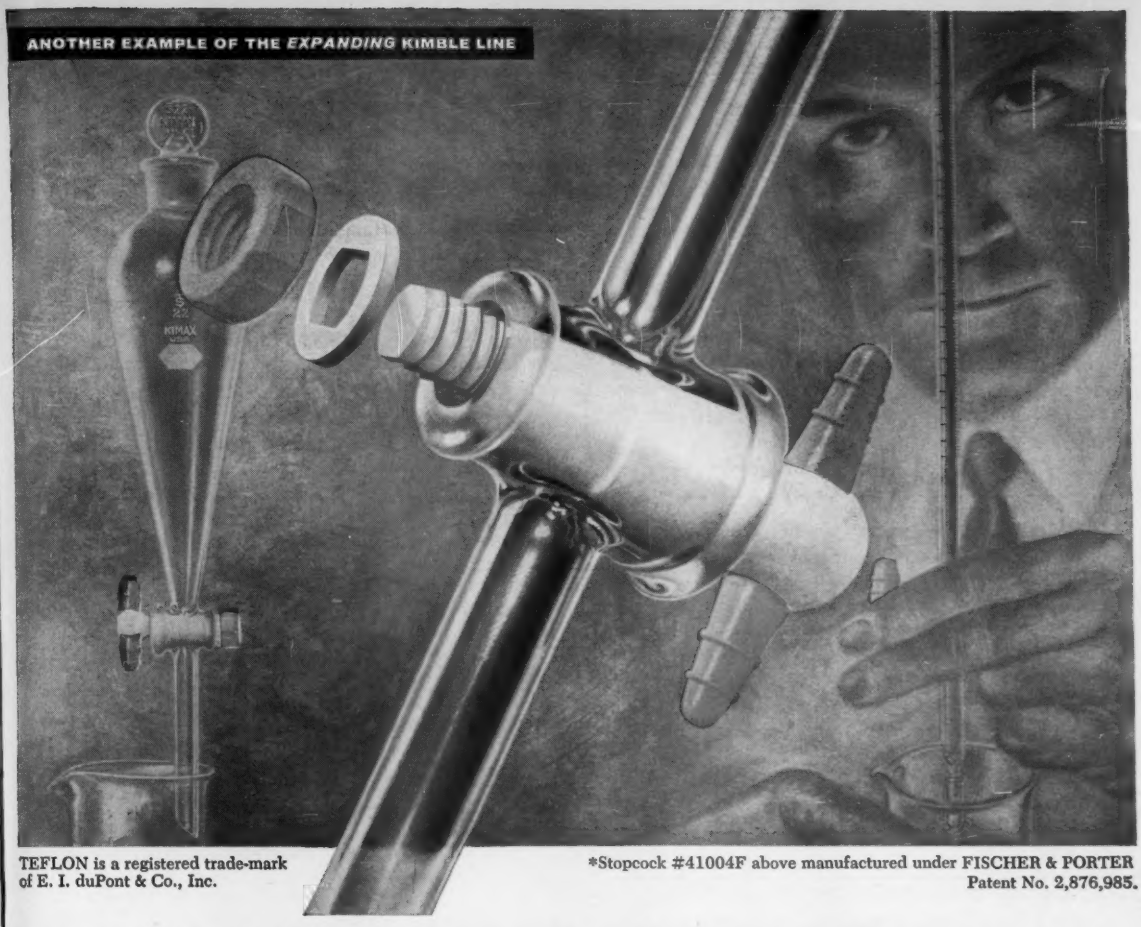
A journey through Lebanon, Syria, Jordan, and Israel during October and November 1959 brought to my attention a significant attitude concerning the United States–U.S.S.R. "space race" and its effect on other peoples as a measure of the prestige position of the two protagonists [see the editorial "Space exploration as propaganda," *Science* **131**, 799 (1960)].

A high percentage of the people with whom I talked could hardly care less. A typical comment was the bitter one that for the cost of one moon shot either great country could irrigate and make fruitful such-and-such a valley. To farmers grimly hoping for enough rain this spring to fill the cisterns on which their farms must depend through the summer ahead, the space race is immaterial.

In evaluating the propaganda effect of such efforts as space exploration, we must not assume that these operations are as important to persons of other cultural backgrounds as they are to us. To a man whose immediate problem is his next meal, the moon is very far away indeed.

JAMES L. SWAUGER

Carnegie Museum,
Pittsburgh, Pennsylvania



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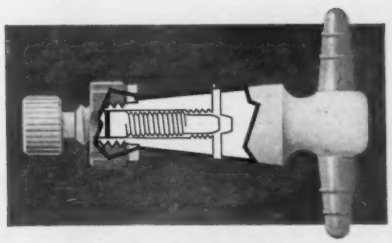

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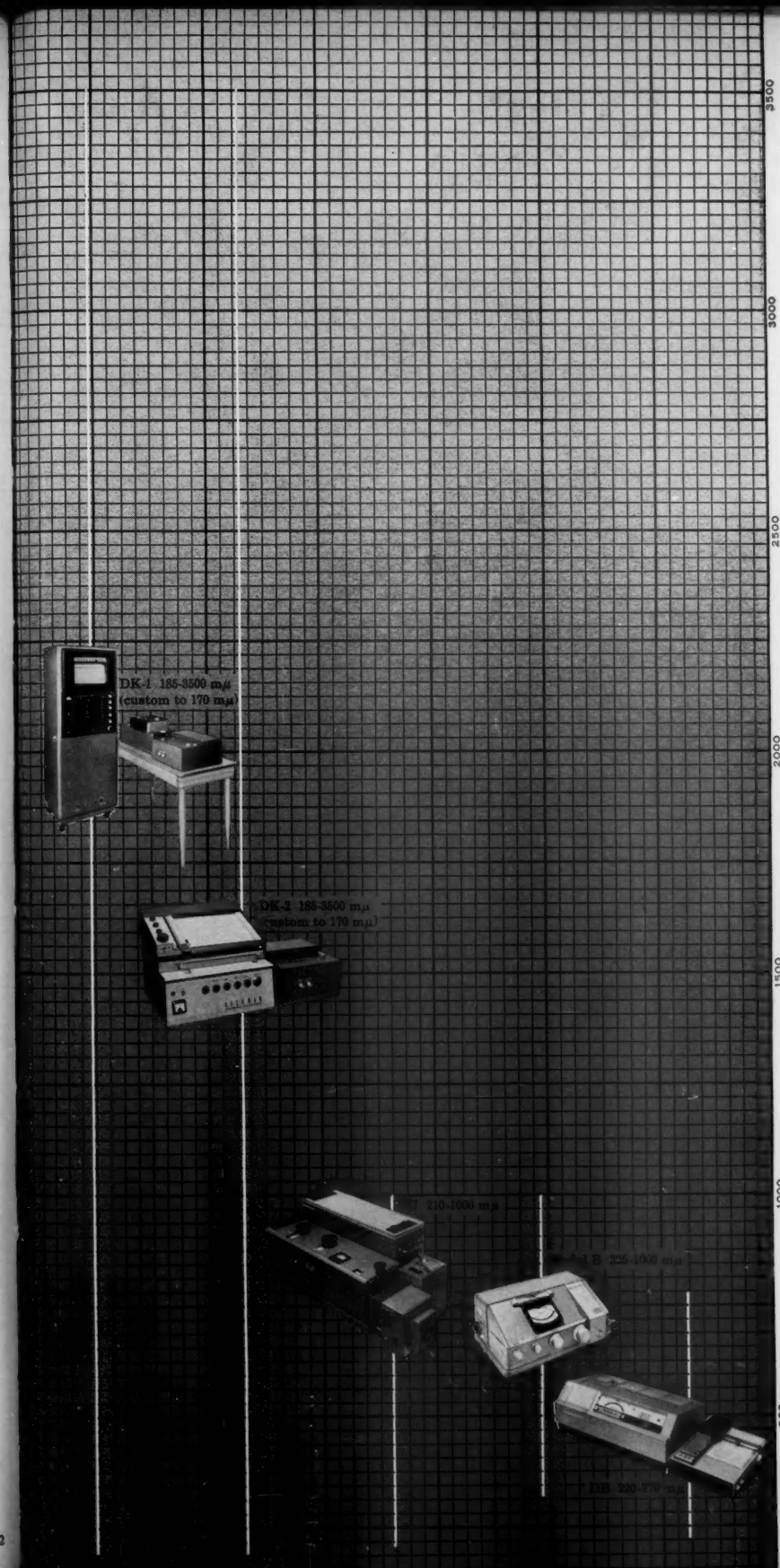

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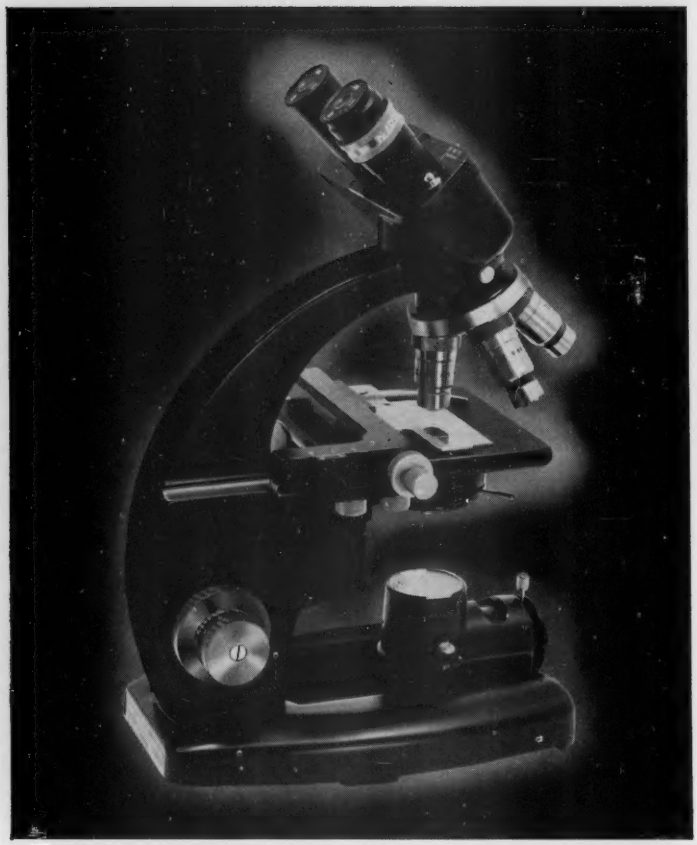

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12 August 1960, Volume 132, Number 3424

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SCIENCE, which is now combined with THE SCIENTIFIC MONTHLY, is published each Friday by the American Association for the Advancement of Science at National Publishing Company, Washington, D.C. The joint journal is published in the SCIENCE format. SCIENCE is indexed in the *Reader's Guide to Periodical Literature*.

Editorial and personnel-placement correspondence should be addressed to SCIENCE, 1515 Massachusetts Ave., NW, Washington 5, D.C. Manuscripts should be typed with double spacing and submitted in duplicate. The AAAS assumes no responsibility for the safety of manuscripts or for the opinions expressed by contributors. For detailed suggestions on the preparation of manuscripts and illustrations, see *Science* **125**, 16 (4 Jan. 1957).

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Annual subscriptions: $8.50; foreign postage, $1.50; Canadian postage, 75¢. Single copies, 35¢. Cable address: Advancesci, Washington.

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A Plank for Science

The main functions of the national conventions of the major political parties are to nominate candidates for the presidency and the vice-presidency and to adopt a platform. We shall here confine our attention to the platforms, which are traditionally statements of party principles and policies. Platforms follow classical lines: the party in power pledges to continue and expand the programs successfully launched under its guidance, excoriates the opposition, praises its own administration, takes credit for the favorable events that occurred during its tenure of office, minimizes setbacks, and paints a grim picture of the evils that would befall the nation should the voters make the mistake of turning control of the government over to the other party. The opposition pledges itself to introduce new legislation to remedy the mistakes of the party in power, to infuse new energy into programs begun under its aegis but allowed to languish during the current administration, excoriates the incumbents, maximizes their reversals, minimizes their accomplishments, and promises to restore the republic to the happy state it enjoyed before it was brought to the brink of ruin by maladministration.

Platforms have been variously described as "something to run on but not to stand on," as "masterpieces of ambiguity," and as documents "full of sound and fury, signifying everything." But they are more than this. Their form is dictated by their function. They are designed to weld the party into unity, and hence must represent the least common denominator of party opinion. They commonly incorporate the major advances made by either party in the past and thus reflect changing concepts of government. A platform is no blueprint for specific action. It is rather an expression of general intent, and as such has something of the force of a moral commitment.

Platforms inevitably reflect current preoccupations of the nation: for some years there have been planks about labor, agriculture, health, foreign policy, domestic affairs, and so on in the platforms of both parties. This year, both Republicans and Democrats have included planks about science. Both parties favor accelerated space research and the international control of space and pursuit of research in atomic energy and medicine. They differ in the amount of detail they devote to basic research and to the role of the government in science. The Democrats "recognize the special role of the Federal Government in the support of basic and applied research" and stress especially an intensified program of reactor research for nuclear power. The Republicans are more specific about basic research: they recognize that "our continuing and great national need is for basic research"; they call for the federal government to support the basic research that industry cannot be expected to pursue. They advocate allowance of reasonable charges for overhead on research contracts, a matter that is not touched upon in the Democratic platform. The Democrats are also silent about the organization of the government for scientific administration and advice, but the Republicans advocate continuation of the President's Scientific Advisory Committee and of the Federal Council for Science and Technology. The Democrats plump for an expanded program in oceanography, a field not mentioned by the Republicans.

That the parties differ in detail is of less significance than that they agree on one major point: for the first time in history both have made the support and organization of science a major plank in their platforms.—G.DuS.

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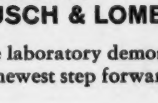

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CURRENT PROBLEMS IN RESEARCH

High Magnetic Field Research

Magnetic pressure can move mountains of metal or plasma for the engineer, and atoms for the physicist.

Harold P. Furth

The opposition encountered in bringing together two like magnetic poles is somewhat like the opposition of a gas to compression between two pistons. This item of common experience has its exact formulation in terms of Maxwell's electromagnetic field theory. Magnetic field is said to possess an energy density which is analogous to the thermal energy density of a gas. When a region of magnetic field is compressed or distorted, its energy content is raised. Accordingly, the action that generates the compression or distortion must be associated with the performance of work against a magnetic pressure, analogous to the thermal pressure of a gas.

To be more specific, the energy density associated with a magnetic field B, measured in gauss, is $B^2/8\pi$, measured in ergs. A magnetic field of 1 megagauss has an energy density of 40×10^6 ergs, or 1000 calories, per cubic centimeter, which is comparable to the chemical energy density of high explosives. The pressure that is exerted at the boundaries of a region of magnetic field B is also given by $B^2/8\pi$, measured in dynes. At 1 megagauss, the magnetic pressure is 40×10^6 dynes, or 560,000 pounds per square inch, which is comparable to the yield points of the toughest metals. For many purposes, what matters is not the maximum pressure that a given magnetic field is capable of exerting but the pressure differential associated with a small local increment or decrement ΔB. Simple algebra shows that the corresponding differential in pressure (and energy density) is given by $B\Delta B/4\pi$.

The author is on the staff of the University of California's Lawrence Radiation Laboratory, Livermore, Calif.

Applications of High Magnetic Fields

The achievement of very high magnetic fields has its obvious uses in pure and applied research. For instance, the confinement of a hot plasma in a "magnetic bottle" (the major problem in the development of fusion power) evidently requires a magnetic pressure at least equal to the pressure of the plasma confined. For reasons of stability, the main trend of fusion research is currently towards magnetic bottles where the plasma creates only a small perturbation ΔB in the magnetic field structure. To confine typically interesting plasma temperatures and densities of 10^9 degrees and 10^{14} particles per cubic centimeter, a magnetic field of 50 kilogauss or so will be needed, and the scaling laws are such that operation at still higher fields becomes increasingly attractive. If there were no problems and expenses involved in bottling the magnetic bottle itself in a system of conductors, the problem of bottling a thermonuclear plasma would be very greatly simplified.

It is instructive to contrast plasma containment in a magnetic bottle with particle containment in a high-energy accelerator. A stream of particles in orbit may be thought of as a loop of current producing a mean magnetic field perturbation ΔB. In a typical accelerator, with guide field B, the interaction energy density $B\Delta B/4\pi$ is infinitesimal compared with $B^2/8\pi$, and therefore the loss of the particle stream by plasma-type instabilities is out of the question. The main limitation is to be found precisely in the weakness of the magnetic interaction force. When conventional iron-core electromagnets are used, as has been the practice, the strength of the guide field is limited to 20 kilogauss, and the centrifugal tendency of the particles can be moderated only by resorting to physically large orbits. The 25-Bev proton accelerators being put into operation at CERN and at Brookhaven National Laboratory have diameters of about 100 meters. The attainment of substantially higher energies along the same technological route is thus limited by obvious financial and geological considerations. An alternative is to employ air-core magnets and begin to operate in the 100- to 500-kilogauss range, thereby reducing the physical scale by a factor of 5 to 25.

High magnetic field research can be divided more or less neatly into two categories. Some branches of research are concerned with high magnetic energy densities as such. An important example is the controlled-fusion project. There are technologically similar projects that concern themselves with the use of magnetic pressure for the propulsion of plasma streams or test projectiles to extreme velocities. The basic line of research in this category is, of course, the technology of high field generation itself, which is discussed in some detail below.

To the second category belong re-

search applications based on the interaction energy density of a high magnetic field interacting not with itself but with infinitesimal perturbation fields. The example of the particle accelerator has been given. A much more general example is the use of high applied fields to interact with microscopic field regions inside matter. The high-field Zeeman effect, and the various galvanomagnetic, paramagnetic, diamagnetic, ferromagnetic, antiferromagnetic, and associated resonant effects, to mention a few, are all dependent on the transcendence of a magnetic interaction energy beyond the reach of some disturbing influence. The magnetic perturbation fields ΔB associated with atomic electron orbits, while confined to extremely small volumes, of 10^{-25} cubic centimeter or so, have large local amplitudes, easily falling into the 10^5 to 10^7 gauss range. The various electron orbits within an atom interact magnetically with each other. The application of an external B sufficiently strong to dwarf the individual ΔB's (and make the energy density $B\Delta B/4\pi$ locally transcend the energy densities $\Delta B^2/8\pi$) evidently calls for at least 1 megagauss. Since atomic volumes are so small, however, the total energy involved in the interaction of an atomic electron with a megagauss field corresponds to something rather less than mean thermal energy at room temperature. Moreover, the magnetic perturbations produced by different atoms tend to be random, and so the macroscopic reaction on the applied magnetic field is usually quite negligible. A familiar exception occurs in the case of ferromagnetic materials, where large conglomerates of atoms may act cooperatively to produce macroscopic fields of some tens of kilogauss. Even then the energy density involved amounts only to a fraction of a calorie per cubic centimeter.

Many magnetic properties of matter depend on the frequency of free electron gyration in an applied magnetic field relative to the frequency of electron collisions with various atomic obstacles. The gyration frequency is proportional to the strength of the applied magnetic field, while the rate of collision depends on the thermal velocity. The ratio of these two numbers determines whether electrons will gyrate in the magnetic field or follow randomly broken paths. For solid-state experiments that depend on the resonant acceleration of electrons in magnetic orbits, the collisional effect is about as disturbing as the presence of air inside a particle accelerator. When conventional field strengths of 20 kilogauss or less are used, such experiments must be conducted at very low temperatures, and a great many cannot be done at all. With megagauss fields, a new range of experimentation becomes accessible, some of it even at room temperature. Evidently, however, there are solid-state experiments that still cannot be done at all, and that await the further development of high-field technology.

The relative feebleness of available magnetic field strengths becomes especially noticeable in relation to the fields produced by the motion of charged particles in the nucleus. Over typical nuclear volumes of 10^{-37} cubic centimeter, there are magnetic fields in the 10^{15} gauss range. It appears unlikely that individual nucleons could be decoupled from each other and coupled to an external field. On the other hand, the total energy of interaction between a nucleus and an applied field of 1 megagauss corresponds to a thermal energy of only a few hundredths of a degree. Accordingly, experiments designed to align nuclei magnetically, in the face of disruptive thermal agitation, must be conducted either at extremely low temperature or in an extremely high applied field. These simple considerations illustrate how the strength of microscopic fields and their minuscule spatial extent provide separate incentives for the development of high-field techniques.

In high-energy physics, the most common use of magnetic fields is that already mentioned of guiding energetic particles along paths that are more or less strongly curved, depending on the field strength and the particle momentum. In many special cases, the use of conventionally weak fields has disadvantages that go beyond the practical inconvenience inherent in very large-scale installations. For example, if the magnetic analysis of secondary beams involving short-lived particles is not performed in as short a distance as possible, the desired beam component may disappear in the process of analysis.

Many high-energy experiments are carried out in media sensitive to the passage of particles, such as photographic emulsions, bubble chambers, or cloud chambers. The medium acts both as a target material and a detector of particle paths. A magnetic field is imposed whenever possible, for the purpose of momentum analysis; but if the medium is very dense and the applied field is conventionally weak, the random scattering of particle paths by collisions will mask their curvature in the magnetic field. Magnetic analysis has been used consistently in cloud chambers and low-density bubble chambers at 20 kilogauss or so but is only now coming into use with emulsion stacks and high-density bubble chambers, at applied fields of a few hundred kilogauss.

Magnetic Field as a "Two-Dimensional Gas"

In order to gain insight into the basic problems of high-field technology, it is often convenient to abandon familiar pictures of magnetic fields and forces produced by currents in wires and to think instead of the magnetic field as a sort of two-dimensional gas, with the flux lines taking the place of gas molecules. This analogy becomes exact for systems that are infinite and uniform along the direction of the field.

Consider, for example, an infinite tube of conductive material, containing an axial magnetic field. If the material has zero electrical resistivity, the field may be confined entirely to the tube bore (Fig. 1*a*), and there will exist an azimuthal surface current "generating" or "generated by" the magnetic field. The radial pressure exerted on the tube surface will be the same as the energy density of the magnetic field—namely, $B^2/8\pi$. By executing an imaginary radial displacement of the tube wall, it is easy to show that the algebraic identity of pressure and energy density is typical of a two-dimensional ideal gas.

If we now assign a finite resistivity to the tube material, the magnetic field will begin to "leak" radially outward, just like a gas confined in a porous vessel (Fig. 1*b*). The current associated with the magnetic field boundary will also spread to the volume of the tube, and like the magnetic field, it can do so only gradually. This is the basis of the "skin effect" familiar in high-frequency electrical engineering. As the magnetic field expands into the tube wall, it does work, and this corresponds to the appearance of Joule heat. The density of the work performed is, roughly speaking, equal to $B^2/8\pi$. Hence, we can calculate the temperature rise at the surface of the tube quite independently of the assumed resistivity, depending only

on the heat capacity per unit volume, which for most solids is unity, at room temperature. When a megagauss field has leaked out, the tube surface temperature will have risen by some thousands of degrees.

We can now state a basic (approximate) scaling law relating to the generation of high fields in solid metal systems: The confinement of a magnetic energy density corresponding to 1 megagauss creates a surface temperature comparable to the melting points of metals, as well as a pressure comparable to their mechanical yield points. The theoretical expectations for soft metals with low melting points, like copper or aluminum, are of course much lower than those for hard metals with high melting points, like tungsten or certain copper alloys. It is only with the latter than one can hope to support megagauss fields without incurring damage.

Magnetic field leakage into a conductor is a diffusion process of the standard sort, with a diffusion constant which is just equal to the resistivity of the conductor. Thus we arrive at a second basic scaling law of high-field technology: The confinement time of a magnetic flux in a system of conductors is proportional to the conductivity and the cross-sectional area of the system. For copper systems of 10-centimeter diameter, the confinement time is roughly 0.1 second.

The diffusion of heat in metals follows the same scaling law as the diffusion of magnetic field, but with a different and usually much smaller diffusion constant. Thus, heat conduction ordinarily has little moderating effect on the high temperatures reached transiently during the leakage of a confined magnetic field out of a solid metal tube.

If we make a radial slit in our field-confining tube (Fig. 1*c*), a voltage can be applied, and additional magnetic flux can be introduced through the slit at a rate equal to the applied voltage. The voltage source may be thought of as the pump which supplies the magnetic "gas" to its tubular reservoir, from which it is continuously leaking. In steady-state operation, the rate of work done by the pump will be equal to the magnetic energy stored in the tube, divided by the leakage time. The power requirement on the pump does not depend on the area of the tube, since the area and the leakage time have the same dimensional dependence. The power required to maintain a 100-kilogauss field in copper at room temperature can thus be expressed simply as about 100 kilowatts per centimeter of length. The steady-state confinement of a 1-megagauss field (which no high-field experimenter thinks seriously of attempting at present) would impose the alarming requirement of 10 megawatts per centimeter.

Transient high-field generation has the great advantage that electrical energy can be accumulated at low mean power in a capacitor bank or a motor generator and then suddenly converted into magnetic energy, producing enormous instantaneous power. If the introduction of the desired magnetic flux into the tube is accomplished in a time that is short compared with the magnetic leakage time, the situation shown in Fig. 1*a* will be approximated. In that case, the energy per centimeter of tube length needed to produce a magnetic field B, is just $B^2/8\pi$ times the inside tube area. If the process of flux introduction consumes several leakage times, much more electrical energy is needed to reach the same peak magnetic field. Thus, it is easy to calculate that, for the sake of efficiency, copper systems of centimeter size must operate in the millisecond range, while systems that are smaller or made of less conductive metals, such as the harder alloys of copper, will do best with still shorter pulse times.

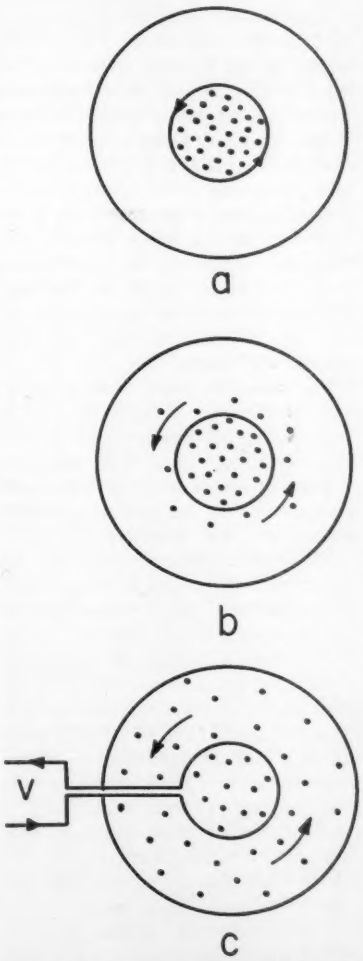

Fig. 1. Cross-section of an infinite flux-confining tube. Arrows represent the current, and dots represent the magnetic field. The magnetic return flux is not shown. (*a*) Initial state; (*b*) outward diffusion of magnetic field; (*c*) steady state.

The cost of producing rapidly deliverable energy is substantial and represents one of the prime limitations on the size and strength of pulsed high-field equipment. At 1 megagauss, the quantity $B^2/8\pi$ corresponds to 4000 joules per cubic centimeter, or at least $500 per cubic centimeter, if the energy comes from a capacitor installation. When the field volume is sufficiently great so that the flux-leakage time can be made to exceed 0.1 second, rotating electrical equipment becomes readily applicable, and the cost per cubic centimeter may drop by an order of magnitude.

In electrically efficient systems, where the situation of Fig. 1*a* is approximated, the rise in surface temperature is held to the theoretical minimum. Slower and less efficient systems imply higher temperatures in the conductor, and steady-state systems tend toward infinitely high temperature. The salvation of direct-current high-field magnets lies in honeycombing the conductor with coolant ducts. The low ratio of thermal to electrical conductivity is then offset by the shortness of the distance through which Joule heat must travel in order to be removed, relative to the distance through which the magnetic flux must leak in order to escape. The technical problems involved in this operation are such that the 100-kilogauss magnet built by Francis Bitter (*1*) in the 1930's still represents a record achievement. A 250-kilogauss direct-current installation (*2*), which was recently designed on the basis of improved rates of transfer of heat to coolant, may go into operation in the next few years.

With the honeycomb cooling technique and the moderate field strengths

that are typical of direct-current high-field magnets, extreme low-temperature operation becomes somewhat attractive. At liquid helium temperature, the conductivity of certain pure metals improves by three orders of magnitude. The flux-leakage time is correspondingly greater, and the direct-power requirement and Joule-heating rate are correspondingly less. At such a low temperature, some materials have ratios of thermal to electrical diffusion constant greater than unity, and the heat-removal problem is thus further eased. On the basis of these ideas, various direct-current magnets with field strengths between 50 and 100 kilogauss have been operated with low-temperature coolants, including liquid hydrogen (*3*) and liquid air. The main drawbacks of this type of operation are the complexity and expense of the equipment and the magnitude of the indirect power requirement for producing the low-temperature coolant. Recent calculations by Post and Taylor (*4*) have shown that very large low-temperature systems are highly profitable, and so we may perhaps look forward to an era of refrigerated high-energy accelerators and fusion reactors.

Some Approaches to High-Field Technology

The design of actual high-field equipment is governed by basic considerations very similar to those developed for the infinite tube of Fig. 1. In systems of increased complexity, certain limitations can be removed. On the other hand, there arise vexatious practical requirements, such as the generation of the circulating current associated with the confined magnetic field. Roughly speaking, 1 gauss is associated with 1 ampere per centimeter of tube length. How to generate currents of some megamperes, as required for a megagauss field volume some centimeters in length, is by no means a trivial problem.

Five main technological avenues have been followed to produce high magnetic fields: (i) reduction of the input current requirement by means of a tubular coil wound of insulated wire; (ii) direct adoption of the solid tube design, with a straight or helical slit; (iii) use of motional effects in solid but nonrigid tubes; (iv) use of conductive fluid; (v) reduction of magnetic forces by means of force-free geometric designs. These techniques are briefly discussed in the following paragraphs.

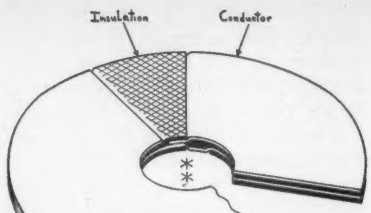

Fig. 2. Slightly modified form of the Bitter coil construction, as employed in a 300-kilogauss pulsed magnet (2.25-inch inside diameter) recently built by Birdsall and Furth at the Lawrence Radiation Laboratory.

Wire-wound coils. The foundations of high-field research were laid in the 1920's by the Russian scientist P. Kapitza (*5*), who was then working at Cambridge, England. His coils were wound of several layers of thick bronze wire, surrounding an experimental volume of several cubic centimeters. By short-circuiting a specially built motor generator, he was able to produce 320-kilogauss pulses in the millisecond range. To judge from Kapitza's large-scale conduct of solid-state experiments (*6*), this installation must have performed very satisfactorily.

For some 20 years after Kapitza's work there were no major innovations in technique. Capacitor banks or low-temperature cooling, or both, were used by some experimenters, but the size and strength of their magnets were modest as compared with Kapitza's.

High-field experimentation on the heroic scale was resumed only in 1954–55, behind the formerly closed doors of the controlled-fusion project "Sherwood." F. H. Coensgen (*7*) developed high-field coils made of copper windings embedded in a Fiberglas-base plastic and was able to reach 450 kilogauss over a a 100 cubic centimeter volume during millisecond times, by discharging a high-voltage capacitor bank. Coils of the same type, of 200-kilogauss strength with 10^3 cubic centimeter volumes, are still in daily use in "mirror machine" work (*8*), and a 70-kilogauss coil of 10^5 cubic centimeter volume is being put into operation.

On comparing the wire-wound coil with the solid tube of Fig. 1, one notes a number of important differences, aside from the obvious reduction in the input current requirement. When there are several concentric layers of windings, the same current must flow through all, and so even a very rapidly pulsed field will appear simultaneously throughout the body of the coil instead of appearing first inside the coil bore and gradually diffusing into the body. A multilayer coil in short-pulse operation is therefore more wasteful of stray magnetic energy than a solid tube. On the other hand, one can show that the enforced distribution of the coil current over the whole coil body makes it possible to reduce the rise in temperature to an arbitrary low if there is a sufficiently short pulse. The danger of melting can thus be removed in a wire-wound coil, but the danger of bursting is always very much aggravated, because of the relative fragility of the composite wire-insulator structure.

Solid metal coils. A practical coil resembling the flux-confining tube of Fig. 1 was first devised by Bitter (*1*) for his direct-current magnets. Bitter's coil is made of alternate copper and mica washers, each having a radial slit. These washers are stacked up artfully to form a solid tube with a helically winding slit (see Fig. 2). The Bitter coil is thus in effect a monolayer multiturn coil.

In 1954, R. W. Waniek and I began a high-field project at the Harvard Cyclotron Laboratory, aimed at providing accessory magnets for accelerator experiments (*9*). In view of the second-rate mechanical properties of wire-wound coil structures, our thinking turned to coils made of more or less solid metal. We began with an effort to exploit the high-field potentialities of Bitter's coil design in combination with Kapitza's pulse technique and the use of hard metals. To obtain the large currents required for coils with a small number of turns, we discharged low-voltage capacitors during 100-microsecond times directly into our coils, or during millisecond times, through a pulse transformer. With a rather modest technological investment, we were able to reach 600-kilogauss fields over 0.1 cubic centimeter volumes by the summer of 1955. Concurrently, we began to put our magnets to use in some simple semiconductor experiments (*10*).

Collaboration was established with the nearby Lincoln Laboratory, where B. Lax and his associates had become interested in applying our high-current, solid-metal technique in their own solid-state research. Various useful innovations were made at Lincoln, notably the

machining of the helical slit directly into a solid tube instead of generation of the slit by an assembly process of the Bitter type. Fields as high as 750 kilogauss over 0.1 cubic centimeter volumes, during 100-microsecond pulse times, were soon reported by S. Foner and H. H. Kolm (*11*). A series of magnetic resonance experiments (*12*) was performed at the 100- to 300-kilogauss level.

In the fall of 1955 we began a fruitful collaboration with M. A. Levine of the Cambridge Air Force Research Center. By means of high-current techniques developed by Levine, we were able to draw multimegampere current pulses of 15-microsecond duration from a capacitor bank. The extremely high input current requirement of the solid, single-turn coil (Fig. 1) could thus be met for the first time. We proceeded to study the field-limiting phenomena peculiar to various metals and to evolve a systematic theory of high-field limitations in the flux-confining tube (*13*), somewhat as outlined in a previous section of this article.

Some of our single-turn coils of 0.1 cubic centimeter volume could be pulsed repeatedly at fields up to 900 kilogauss, with only minor surface damage. At still higher fields we began to find ourselves in a new domain, where inertia rather than tensile strength provided the main opposition to the magnetic pressure (see Fig. 3).

The maximum distance over which a flux-confining tube can expand in a given time can be estimated by equating the kinetic energy density reached by the tube material to the work done by the magnetic pressure in the process of expansion. A megagauss field in a solid copper system of centimeter dimensions can thus be estimated to last for times in the 10-microsecond range, even when the mechanical strength of the copper is neglected. (Since the tensile strength of copper is one-tenth the value of $B^2/8\pi$ at 1 megagauss, it may indeed be neglected.) In our experiments with 15-microsecond pulses, we found that peak fields as high as 1.6 megagauss could readily be reached. At the end of a typical pulse, the inside coil diameter would be distended to some five times its original value of about 0.3 centimeter.

Fig. 3. End view of a single-turn coil made of beryllium copper, after pulsing at 1.2 megagauss. The coil expanded from ¼ to ⅜ inch in inside diameter and suffered surface melting.

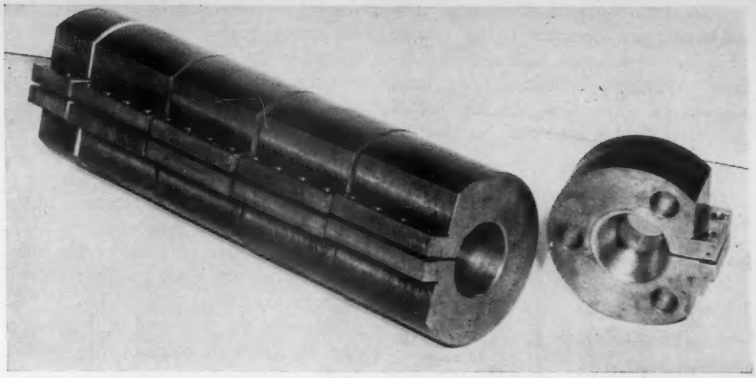

Fig. 4. Steel single-turn coil of 3-centimeter inside diameter, used for plasma compression at the Naval Research Laboratory. Inside the "magnetic mirror" end piece, field strength of 600 kilogauss has been reached.

During recent years, our rather primitive experiments with the solid flux-confining tube have been surpassed in many ways, both in technique and dimension. Figure 4 shows a single-turn coil used in "magnetic mirror" plasma experiments by A. Kolb (*14*) at the Naval Research Laboratory. In the mirror regions at each end, 600-kilogauss fields have been achieved nondestructively over a volume of many cubic centimeters, during 15-microsecond pulse times. Hardened steel is used as the coil material, and many megamperes of current are supplied from a low-inductance capacitor bank.

The highest fields to be obtained nondestructively thus far have been achieved at the Zenith Radio Research Corporation by W. W. Salisbury and L. C. Foster (*15*), and have not yet been reported in the literature. The method is based on the solid-metal single-turn coil principle, with the added feature that the input current is supplied by a wire winding, tightly surrounding the single turn. This transformer-like arrangement, which is sometimes called a "flux concentrator," has the merit of reducing the input current requirement, though usually at the expense of some electrical efficiency. Starting in 1956, on a program involving considerable metallurgical research, Salisbury and Foster found that, with certain high-conductivity bronzes and proper shaping of the inside coil surface, they were able to reach fields as high as 1.6 megagauss repeatedly in the same coil.

During the past year, some very sophisticated flux-concentrator designs that combine the virtue of high electrical efficiency with that of low input current requirement, have been developed at the Lincoln Laboratory.

On the basis of these various technological contributions, it appears that the use of reliable and convenient pulsed megagauss magnets may well become commonplace in physics and engineering during the next decade.

Coils with mobile walls. The highest magnetic fields ever generated in the laboratory were recently reported by C. M. Fowler, W. B. Garn, and R. S. Caird (*16*), at Los Alamos. A thin copper flux-confining tube was imploded by means of high explosives, and a large volume of initially weak magnetic field was compressed up to a strength of 14 megagauss in a volume of about 2 cubic centimeters. The attainment of such a prodigious magnetic field, and its duration near peak for about 2 microseconds, is ascribable to the inertia of the imploded system.

This technique of imploding tube walls that are designed to be mobile (Fig. 5*a*) is superior to the simple delayed-expansion technique (*13*) in a number of ways: first, the interval of maximum field is extended, because the inward motion must be arrested before expansion begins; second, the energy stored in the magnetic field is supplied principally from the chemical energy of the high explosive, rather than from expensive sources of fast electrical energy; and third, the input current requirement is small, since the current is amplified during the compression to the same extent as the magnetic field itself. These points are of considerable practical importance, since the energies and currents associated with the Los Alamos experiment are measured in megajoules and hundreds of megamperes, respectively.

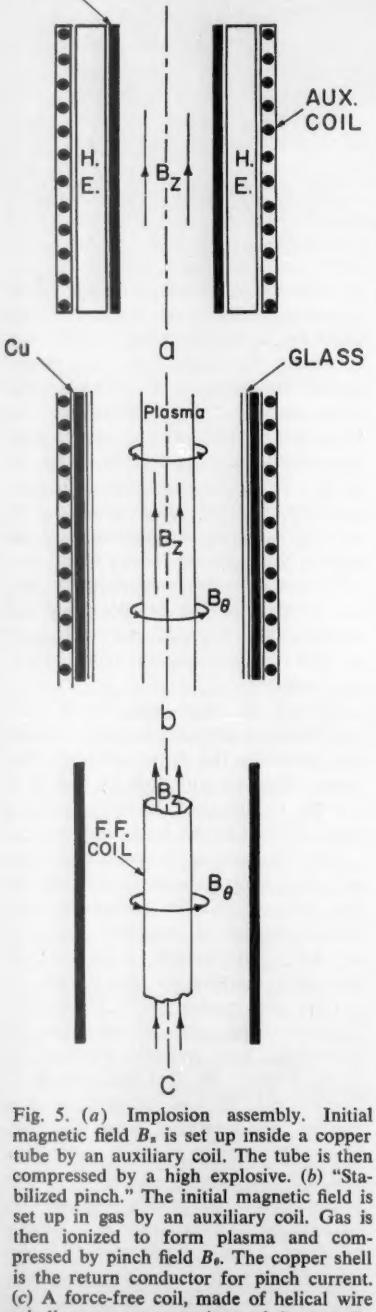

Fig. 5. (*a*) Implosion assembly. Initial magnetic field B_z is set up inside a copper tube by an auxiliary coil. The tube is then compressed by a high explosive. (*b*) "Stabilized pinch." The initial magnetic field is set up in gas by an auxiliary coil. Gas is then ionized to form plasma and compressed by pinch field B_θ. The copper shell is the return conductor for pinch current. (*c*) A force-free coil, made of helical wire windings separates regions of B_z and B_θ. The outer shell is the return conductor for coil current.

There exist quite a few "mobile-wall" experiments that are similar to the outstanding example that has been given. A common idea is, for instance, to generate large electrical energies and currents for outside use in an imploded tube with a slit and attached leads. The firing of a cannon ball through a high-field coil has been suggested for the same purpose, and conversely, the firing of a cannon ball *by* a high-field coil is a concept that dates back half a century.

Use of conductive fluid. From mobile "solid" conductors it is but a short step to the use of conductive fluid, such as plasma or liquid metal. The compression and heating of plasma by means of a fast-rising magnetic field is a common practice in controlled-fusion research. When a significant fusion power release has been achieved, the converse process—that of hot plasma expansion compressing the initial field—will increase magnetic energy in a manner analogous to the generation of magnetic from chemical energy in the Los Alamos implosion experiment. This is the basis of the statement that electrical energy can be generated directly by a fusion reactor.

In the "stabilized pinch" (*17*), illustrated in Fig. 5*b*, an axial magnetic field inside a plasma is compressed by the azimuthal pinch magnetic field. By this technique, fields in the 100-kilogauss range have been achieved for fractions of a microsecond. Construction of practical high-field magnets based on plasma does not seem very likely at present, because of the low electrical conductivities (0.001 that of standard copper) which have been achieved thus far. To obtain a conductivity comparable to that of copper (theoretically possible at 10 million degrees) appears to be about as difficult a problem as the control of fusion power itself. The main obstacle in both cases is that of instability phenomena.

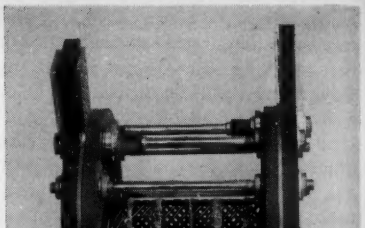

Fig. 6. A force-free coil built at Livermore Radiation Laboratory. The current flows along helical wires and returns through rigid outer rods (which are not force-free). Copper end plates (sandwiched between plastic) bear the main pressure caused by the pulsed field.

The use of a "flux-confining tube" consisting of radially inward-moving liquid metal has recently been suggested by H. H. Kolm as a means of generating strong, steady magnetic fields. With this ingenious technique (as indeed also with the Los Alamos implosion technique) one would again expect the ultimate limitations to come from instability phenomena.

Force-free coils. In order to avoid altogether the vicissitudes experienced in seeking to contain extreme magnetic field pressures, a fifth possible approach may be adopted (*14*). By winding coils three-dimensionally, instead of merely azimuthally, a three-dimensional magnetic field, instead of a purely poloidal one, can be produced. As has been found in plasma physics, a three-dimensional magnetic field and current structure can be made force-free, provided that exterior boundary conditions are met. These boundary conditions imply that a region of finite magnetic energy density corresponds to a finite total outward force at the boundary of the force-free system. The outward pressure can, however, be made arbitrarily small by placing the boundary at a sufficient distance. This is illustrated by the example of the "stabilized pinch," where the "pressures" of the axial and azimuthal fields are balanced against each other. If the plasma is kept cold by loss mechanisms, the magnetic field exerts no net pressure at all, except against the current-return-conductor encasing the pinch tube. As is well known,

the strength of the azimuthal magnetic field set up by passage of a current along a cylinder drops off in proportion to the radial distance from the cylinder axis. By making the ratio of tube radius to pinch radius sufficiently large, the magnetic field strength at the wall can be made lower than that at the center of the pinch to the extent desired.

In seeking to produce force-free laboratory coils, it is natural to begin with structures approximating the "stabilized pinch" (Fig. 5*c*). A principal problem is how to terminate such structures in the axial direction. One possible approach is to imitate the toroidal "stabilized pinch," where there are no end effects. A number of equally simple solutions exist, resembling conventional coils more closely, and some of these have been built and tested at the Cambridge Air Force Research Center and at Livermore (Fig. 6).

Fragile elements have been used intentionally in coil construction, and structures readily deformable by hand have been found to withstand pulsed fields in the range of 100 to 200 kilogauss. The main mode of deterioration at present is again the same as in plasma high-field experiments—namely, the loss of the equilibrium configuration by an unstably growing geometric perturbation. The obvious remedy is to improve the accuracy with which laboratory models are made to meet theoretical designs, or to reinforce approximately force-free coils mechanically, thus preventing the initial growth of instabilities.

Properly made force-free coils will be wound in a great many layers, and, as in the case of ordinary multilayer coils, it will be possible to reduce the rise in temperature to an arbitrary low if the pulse is sufficiently rapid. Force-free coils are in principle exempt from both of the main high-field limitations —that due to melting and that due to mechanical deformation.

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Exobiology: Approaches to Life beyond the Earth

Joshua Lederberg

It is a privilege to discuss some basic problems in biology with an audience whose special concern is for the recent striking advances in the physics of the earth in the solar system. However, many of us are looking forward to the close investigation of the planets, and few inquisitive minds can fail to be intrigued by what these studies will tell of the cosmic distribution of life. To conform to the best of our contemporary science, much thoughtful insight, meticulous planning, and laboratory testing must still be invested in the experimental approaches to this problem. This may require international cooperation and also—perhaps more difficult—mutual understanding among scientific disciplines as isolated as biochemical genetics and planetary astronomy.

Many discussions of space exploration have assumed that exobiological studies might await the full development of the technology for manned space flight and for the return of planetary samples to the terrestrial laboratory. To be sure, these might be preceded by some casual experiments on some instrumented landings. One advantage of such a program is that time would allow exobiological experiments to be planned with composure and deliberation. Undoubtedly, this planning would be more rigorous insofar as it was based on improved knowledge, from closer approaches, of the chemistry and physics of planetary habitats. Unfortunately, this orderly and otherwise desirable program takes insufficient account of the capacity of living organisms to grow and spread throughout a new environment. This unique capacity of life which engages our deepest interest also generates our gravest concerns in the scientific management of missions beyond the earth. On account of these, as well as of the immense costs of interplanetary communication, we are obliged to weigh the most productive experiments that we can do by remote instrumentation in early flights, whether or not manned space flight eventually plays a role in scientific exploration.

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This article is adapted from a paper presented 13 January 1960 at the 1st International Space Science Symposium, Nice, France—a symposium sponsored by the Committee for Space Research of the International Council of Scientific Unions. The article will appear as chapter IX of the Space Science Board's copyrighted report-in-progress on "Science in Space," and is published here by permission of the National Academy of Sciences.

Motivations for Exobiological Research

The demons which lurk beyond the Pillars of Hercules have colored the folklore and literature of ages past and present, not always to the benefit of fruitful exploration and dispassionate scientific analysis. Apart from such adventuresome amusements and the amateur delights of a cosmically en-

larged natural history, how does exobiology relate to contemporary science and culture? The exploration of space may seem to have very little to do with fundamental questions in biology or medicine, with the role of genes in embryological development, protein synthesis, the biology of viruses, and the evolution of species. The physical sciences may sharpen our perspective. Twenty-five centuries of scientific astronomy have widened the horizons of the physical world, and the casual place of the planet Earth in the expanding universe is a central theme in our modern scientific culture. The dynamics of celestial bodies, as observed from the earth, is the richest inspiration for the generalization of our concepts of mass and energy throughout the universe. The spectra of the stars likewise testify to the universality of our concepts in chemistry. But biology has lacked tools for such extension, and "life" until now has meant only terrestrial life. This disparity in the domains of the physical versus the biological sciences attenuates most of our efforts to construct a theoretical biology as a cognate of theoretical physics or chemistry. For the most part, biological science has been the rationalization of particular facts, and we have had all too limited a basis for the construction and testing of meaningful axioms to support a theory of life. At present, perhaps the only potentially universal principle in biology is the Darwinian concept of evolution through the natural selection of random hereditary fluctuations.

Some chemical attributes of terrestrial life might support a claim to be basic principles: for example, polyphosphates (adenylpyrophosphate) occur in all organisms as coupling agents for the storage and transfer of metabolic energy. But, at least in principle, we can imagine that organisms may have found alternative solutions to the same problem. Only the perspective of comparative biology on a cosmic scale could tell whether this device is an indispensable element of all life or a particular attribute of its local occurrence on this planet.

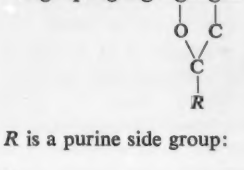
An important aim of theoretical biology is an abstract definition of life. Our only consensus so far is that such a definition must be arbitrary. If life has gradually evolved from inanimate matter, the demarcation of chemical from biological evolution is one of useful judgment. For a working principle, we might again rely upon the evolutionary concept: a living system has those properties (of self-replication and metabolism) from which we may with more or less confidence deduce an evolutionary scheme that would encompass self-evidently living organisms. But I do not propose this as a rote formula for the assessment of life on other celestial bodies, and certainly not before we have some empirical knowledge of the diversities of chemical evolution.

From this standpoint, the overriding objective of exobiological research is to compare the over-all patterns of chemical evolution of the planets, stressing those features which are globally characteristic of each of them.

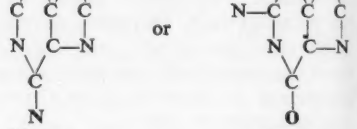
We are all thinking of the question: "Is there life on Mars?" To answer it may require a careful reassessment of our meaning of "life" and matching this with the accumulation of hard-won evidence on the chemical composition of that planet. On the other hand, we might be confronted with an object obviously analogous to an earthly plant, animal or microbe. But even this abrupt answer would be trivial in deference to a biochemical analysis of the organism and of its habitat for comparison with the fundamentals of terrestrial life.

In our first approaches to the nearby planets we will wish to design experiments which have some tangible foundation in the present accumulation of biochemical knowledge. The aqueous environment, and its corollary of moderate temperatures in which large carbonaceous molecules are reasonably stable, are implicit in terrestrial biochemistry. This is not to reject the abstract possibility of nonaqueous life, or noncarbonaceous molecules that might characterize temperatures of $< 200°$ or $> 500°$K. However, we can defer our concern for such exotic biological systems until we have got full value from our searches for the more familiar, and have learned enough of the exotic chemistry to judge how to proceed.

Within the bounds of its aqueous environment, what are the most nearly universal features of terrestrial life? In fact, our plants, animals, and bacteria share a remarkable list of biochemical components, and a biochemist cannot easily distinguish extracts of yeast cells and beef muscle. Among these components, the nucleic acids warrant first attention. Although they constitute the hereditary material, so that all the variety of terrestrial life can be referred to subtle differences in the nucleic acids, the same basic structure is found in the nuclei of all cells. This a long, linear polymer fabricated from a sugar-phosphate repeating unit:

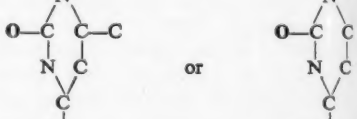


where R is a purine side group:



Adenine or Guanine

or a pyrimidine side group:

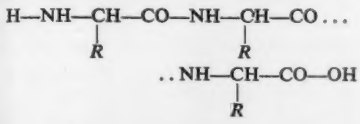


Thymine or Cytosine

The meaningful variety of nucleic acids depends on the specific order of the side group attached to each sugar on this monotonous backbone, a linear message written in a language of four letters, *A*, *G*, *T*, and *C*. The bacteria, which are the simplest free-living organisms, contain nucleotide sequences about 5 million units long; man contains sequences about 5000 million units long—this content being one of our best objective measures of biological complexity. On the other hand, the simplest viruses, which can multiply only inside living cells and come close to being single genes, have about 2500 units per particle. Playing a central role in the unification of terrestrial biology, nucleic acids underlie both heredity and (through their control of protein synthesis) development. Are they the only linear polymers which can subsume these functions, or have many other fundamental types evolved, to be found on other celestial bodies?

Equally general among the constituents of living cells are the proteins, which are also polymers, but with a more diverse set of constituents, some

20 amino acids. The fundamental backbone of a protein is a poly-amino acid chain:



where *R* may be any of 20 different groups, distinguishing a like number of amino acids found in natural proteins. Proteins assume a wide variety of three-dimensional shapes, through coiling and cross-linking of the polymer chains. They are in this way suited to perform such diverse functions as those of enzymes, structural elements, and antibodies. Not only do we find just the same 20 amino acids among the proteins of all terrestrial organisms but these are all the levo- isomers, although dextro- amino acids are found to have other metabolic functions. Next only to the incidence of nucleic acids, we would ask whether exobiota make analogous use of proteins, comprising the same amino acids, in hopes of understanding what seem to be random choices in the sculpture of our own living form.

Common to all forms of terrestrial life are also a number of smaller molecules which are involved in the working metabolism of the cells; for example, most of the B vitamins have a perfectly general distribution. They are vitamins for us only because we have learned, in our evolutionary history, to rely on their production by green plants, rather than to synthesize them within our own cells. But once formed, these vitamins, and similar categories of substances such as porphyrins, play entirely analogous roles in the metabolism of all cells.

A few substances, such as the steroid hormones, do play special roles in the metabolism of higher organisms, and testify to some progress in biochemical evolution. In fact, most objective evidence points to a loss of specific functions—microorganisms are certainly more versatile and less dependent than man is on a specific nutrient mileu. The main trend of biochemical evolution, from microbe to man, has been far less the innovation of new unit processes than the coordination of existing processes in time and space.

While we propose to give first priority to these most general questions, they by no means exhaust our interest in the peculiarities of extraterrestrial organisms, any more than they would for a newly discovered phylum of the earth's own repertoire. Nor should we preclude the possibility of finding new organisms that might be economically useful to man, just as new organisms were among the most fruitful yields of geographic exploration. However, the enlargement of our understanding, rather than of our zoos and botanical gardens, is surely our first objective.

Theories of the Origin of Life

At this point, a consideration of contemporary theory on the origin of life is justified for two reasons: (i) exobiological research gives us a unique, fresh approach to this problem, and (ii) we can find some basis to conclude that life need not be so improbable an evolutionary development as had once been supposed.

The interval between Pasteur's work on spontaneous generation and the recent past has been especially difficult for the mechanistic interpretation of the origin of life. Before Pasteur's time, many investigators could believe that simple microorganisms arose spontaneously in nutrient media. His demonstration that such media remained sterile if properly sterilized and protected seemed to rule out any possibility of "spontaneous generation." His conclusion was, of course, overdrawn, since life must have evolved at least once, and the event could still occur, though very much less frequently than had been supposed before. Meanwhile, the problem was compounded by the growth of biochemical knowledge. We now realize that bacteria, small as they are, are still extremely complex, well-ordered, and representative organisms. The first organisms must have been far simpler than present-day free-living bacteria.

With the growth of genetics since 1900, and the recognition of the self-replicating gene as the elementary basis of life, the question could focus on the origin of the first genetic molecule: given the power of self-replication, and incidents of stochastic variation, Darwin's principle could account for the eventual emergence of any degree of biological complexity.

An immense amount of fruitful genetic work was done in a period when "genetic molecule" was an abstraction and "self-replication" was an axiomatic principle whose chemical basis seemed beyond the grasp of human understanding. Now we recognize that the nucleic acids are the material basis of heredity, and we can begin to construct mechanistic models of their replication. The first principle, as already stated, is that the gene is a string of nucleotides, each position in the string being marked by one of the four nucleotide units *A*, *T*, *C*, and *G*. The polymerization of such strings by the union of the monomeric units presents no fundamental problems, but self-replication would necessitate the assembly of the units in a specific order, the one dictated by the order of the nucleotides in the parent molecule. The key to the solution of this problem was the realization by Watson and Crick that the complete nucleic acid molecule is a rigid, duplex structure in which two strings are united. In that rigid structure, as can be shown by suitable molecular models, adenine occupies a space which is just complementary to that of thymine, and cytosine is likewise complementary to guanine. A string can therefore replicate—that is, direct the assembly of another daughter string—in the following way. The nutrient mix of the cell contains all four nucleotide units. However, at any position of the parent nucleic acid molecule only one of these four can make a suitable fit and will therefore be accepted. After being accepted, the daughter units are firmly bound together by new chemical linkages giving a well-defined daughter string. Kornberg has reconstructed most of these events in some detail, by means of extracts from bacteria, to the very verge of proving duplication of genes in a chemically defined system in the test tube.

However, the media in which such syntheses can occur, in the cell or even in the test tube, are extremely complex. Knowing that the simplest organisms would be the most dependent on their environments for raw materials, where did these precursors come from before living organisms had evolved the enzymes to manufacture them?

Thanks to the insight of Haldane, Oparin, Horowitz and others, we now realize that this paradox is a false one, though it dates to the confusion between "carbon chemistry" and "organic chemistry" which still exists in English terminology. In fact, in 1828, Wöhler had already shown that an organic

compound, urea, could be formed experimentally from an inorganic salt, ammonium cyanate. A hundred years later, a number of routes for synthesis of geochemically significant amounts of complex organic materials were pointed out, for example, the hydrolysis of metallic carbides, and subsequent reactions of olefins with water and ammonia. More recently, Miller and Urey demonstrated the actual production of amino acids by the action of electric discharges on gas mixtures containing the hydrides NH_3, OH_2, and CH_4. This demonstration converges with other argument that the primitive atmosphere of the earth had just such a reduced composition, becoming oxidized secondarily (and in part through photosynthetic separation of carbon from oxygen).

An alternative theory of origin of carbonaceous molecules is even more pervasive. Perhaps we associate carbon with life, and rocks and metals with physical phenomena; beyond doubt we tend to connote the latter with the predominant substance of the universe. In fact, as a glance at tables of cosmic abundance will show, the lighter elements are by far the most prevalent, and after the dispersed hydrogen and helium these are carbon, oxygen, and nitrogen. The primitive condensation of free atoms to form the interstellar smoke, and eventually the stars themselves, must entail the molecular aggregation H + C + O + N; that is, a large fraction of the condensed mass of the universe must consist, or once have consisted, of organic macromolecules of great complexity. The chief problem for their synthesis is in fact not a source of chemical energy but how to dissipate the excess energy of reactions of free atoms and radicals.

This aspect of astrophysics may have place for a remote biological analogy: Once a few molecules have formed, the energy of subsequent impacts can be dissipated among the vibrational degrees of freedom. That is, such molecules can function as nuclei of condensation. As seeds for further condensation, those molecules will be favored which (i) most readily dissipate the energy of successive impacts and (ii) can undergo molecular fission to increase the number of nuclei. The actual molecular chemistry of the interstellar (or prestellar) smoke is thus subject to a kind of natural selection and cannot be a purely random sampling of available atoms.

Whether the earth has retained remnants of this chemistry is hard to say. There is at least some evidence of it in the spectra of comets, and fragments from these continue to form part of the meteoroidal infall. These particles, unless associated with larger meteorites, would be unrecognizable after traversing the earth's atmosphere; they are among the possible treasures to be found buried in protected crevices on the moon.

Light traversing the interstellar smoke has been found to be polarized. If primitive aggregation plays some role in furnishing precursors for biological evolution, this polarization furnishes at least one bias for a decision between levo- and dextro- isomers.

At any rate, possible sources of probiotic nutrition no longer pose a problem. Before the appearance of voracious organisms, organic compounds would accumulate until they reached equilibrium with thermal and radiative decomposition, from which the oceans would furnish ample protection. Locally, the concentration of the soup would be augmented by selective evaporation, and by adsorption onto other minerals. The main gap in the theory, not yet bridged by any experiment, is the actual formation of a *replicating* polymer in such a morass. We are beginning to visualize the essential conditions for chemical replication, and its ultimate realization is foreshadowed both by biochemical studies of nucleic acids and by industrial syntheses of stereospecific polymers.

There is some controversy over whether nucleic acids were the first genes, partly because they are so complex, partly because their perfection hints at an interval of chemical evolution rather than one master stroke. The advantage of the nucleic acid hypothesis is that no other self-replicating polymers have so far been found. But, as an alternative speculation, a simplified protein might replicate by the complementary attachment of acidic versus basic units, perhaps the crudest possible method of assembly. The nucleic acids would be perfections on this theme for replication. The existent proteins do not replicate; with their variety of amino acids, they would have evolved as better adaptations for assuming specific shapes. A comparative view of independent evolutionary systems may at least serve to check such speculations.

Although many steps in the generation of living molecules remain to be re-created, we can state this as a relevant problem for exobiological study, with considerable optimism for the prevalence of life elsewhere. But a sterile planet, too, would be of extraordinary interest to biology for the insight it should give on the actual progress of probiotic chemical evolution.

Natural and Artificial Panspermia

In the foregoing discussion it was tacitly assumed that the evolution of planetary life was a local phenomenon, independent of the incidence of life elsewhere. But, at a time when *de novo* generation seemed less plausible than it does now, Arrhenius defended another hypothesis: *panspermia,* the migration of spores through space from one planet to another. The credibility of the panspermia hypothesis has been eroded mainly for two reasons: (i) the lack of a plausible natural mechanism for impelling a spore-bearing particle out of the gravitational field of a planet as large as the earth, or any planet large enough to sustain a significant atmosphere, and (ii) the vulnerability of such a particle to destruction by solar radiation. In any case, the panspermia hypothesis could be disparaged for evading the fundamental problem by transposing it to an unknown, perhaps scientifically unknowable, site. These difficulties have impeached the standing of panspermia as an experimentally useful hypothesis, but not its immense significance for cosmic biology. In its defense, it might be indicated that, in view of the dormancy of microorganisms in high vacuum and at low temperatures and of their relatively low cross section for ionizing radiations, the hazards of exposure to space may have been exaggerated. The chief hazard to microorganisms might come from solar ultraviolet radiation and the proton wind, but a thin layer of overlying material would shield a spore from these. For the impulsion of particles we might possibly appeal to impacts with other heliocentric bodies, be they grazing meteorites or planetoids in cataclysmic encounters—suggestions not more remote than those invoked for other astronomical phenomena. Nor can we be sure that all the electrokinetic mechanisms which Arrhenius may have had in mind can be excluded from applying to any single particle.

In testing for panspermia, we would be concerned first of all for evidence of interplanetary transport of any material. The moon suggests itself as a nearby trap for particles of terrestrial origin, among which living spores or biochemical fragments of them, might be the most characteristic markers. At one spore per kilogram of sample (a weight ratio of 10^{-15}), the sensitivity of easy biological detection would partly compensate for the vulnerability of spores to physical hazards.

The development of rocket-impelled spacecraft has, of course, furnished a mechanism for producing artificial panspermia. Several authors have recently revived Haldane's passing suggestion that life might even have been disseminated by intelligent beings from other stellar systems. That another century of productive science and technology could give the human species this capability would be hard to dispute. The hypothesis is connected with the age or agelessness of the universe, and until we have a basis for decision on this point, and can make independent tests for intelligent life elsewhere, it must join natural panspermia in the limbo of irrefutable, untestable scientific hypotheses. The technique for attempted radio communication with nearby stars has been detailed recently by Cocconi and Morrison.

These new tools for the exploration of the universe have caught many of us unawares, and few can pretend to have recaptured their equilibrium in dealing with these concepts. Irrefutable notions have little scientific value unless they lead to attempts at verification. A priori arguments for the presence or absence of intelligent life on the planets or in nearby stellar systems are equally unconvincing. The skepticism of most scientists is justified not by conviction but by the consistency of negative evidence in the limited scientific data that have so far been collected.

Planetary Targets

The suitability for life of the accessible bodies of the solar system has already received ample attention. Mars is, of course, the likeliest target, most nearly resembling the habitat of the earth. The indicated scarcity of free moisture and oxygen would severely limit the habitability of Mars by man or most terrestrial animals. However, there seems little doubt that many simpler, earthly organisms could thrive there. Indeed, many students have concluded that Mars does have a biota of its own. The most pertinent evidence is perhaps the infrared reflection spectrum recorded by Sinton which indicates an accumulation of hydrocarbonaceous materials in the dark areas. This is complemented by Dollfus' report (see this symposium) on the seasonal changes of granularity of these areas. The main reservation that must be registered is that these might be meteorological phenomena involving masses of material which may be carbonaceous but not necessarily living. Most such material on the earth's surface is associated with life. However, this may be connected with the greedy utilization of such compounds by organisms rather than their production by vital synthesis. However, the most plausible explanation of the astronomical data is that Mars is a life-bearing planet. (The term *vegetation* is often used; this should be discouraged if it implies that the Martian biota will necessarily fall into the taxonomic divisions that we know on earth.)

The habitability of Venus is connected with its temperature, a highly controversial subject. Perhaps the most useful first contribution to the exobiology of Venus would be a definitive measurement of its temperature profile. Even should the surface be unbearably hot, this need not preclude a more temperate layer at another level.

The exposure of the moon's surface to solar radiation and the absence of a significant atmosphere have led scientists to discount the possibility of a lunar biology. However, the composition of the moon's deeper layers, below even a few meters beneath the surface, is very much an open question (see Urey, this symposium) particularly in the light of Kozyrev's recent reports of gaseous emissions. Realistic plans for the biological study of the moon probably must await the results of chemical analyses. Apart from the remote possibility of indigenous life, the moon is a gravitational trap for meteoroidal material. We may eventually be able to screen large quantities of this virgin material for what Haldane called astroplankton, in an empirical test of the panspermia hypothesis. While exposed deposits would be subject to solar degradation, shaded refuges must also exist. Mercury may be analogous to the moon, except insofar as its dark side may furnish an even more reliable, though much more remote, refuge of this kind.

It may be academic to discuss the exploration of the major planets, in view of their distance and the difficulty of deceleration in the Jovian field. However, their wealth of light elements, subject to solar irradiation at temperatures and in gravitational fields very different from the earth's, offers the most exciting prospects for novel biochemical systems.

Experimental Approaches

A realistic view of our limitations requires that our treatment of this topic be one of utmost humility. Useful landings on planetary targets are fraught with difficulties and hazards, and experiments done at a distance must not be overlooked in the excitement of planning for more adventurous missions. Balloon- and satellite-mounted telescopes can tell much about planetary chemistry, and hence biology, and probes to the vicinity of a planet can furnish additional information prior to actual landing.

It is instructive to ask ourselves how we might diagnose the existence of life on the earth from distant observations. If we may judge from the photographs so far obtained from high-altitude flights, we could hope to detect only large-scale manifestations of organized culture—cities, roads, rockets. This reserve may not give due credit to the possibilities of high-resolution photography and sensitive infrared spectrometry, and reasonable implications from seasonal changes in the color and texture of terrain. However, we may conclude that distant approaches will be invaluable for deriving preliminary chemical information but probably will not be decisive for exobiological inferences. Even if we could more surely decide that the Martian cycle involved living organisms rather than inanimate chemical transformations, we would still have little insight into the intimate biochemical details which are a major objective of exobiological research. On the other hand, a planet could harbor an extensive biota that would defy detection from a distance, like the biota of our own extensive deserts and deep waters.

Microorganisms, for many reasons, are the best prospects on which to concentrate marginal capabilities. They are more likely to flourish in a minimal

environment than larger organisms. The microbes must also precede the macrobes in evolutionary sequence, though we must not suppose that present-day bacteria are necessarily very primitive. The earth is well endowed with both kinds of organisms; we can imagine another world with only microbes, but we cannot conceive of one lacking microbes if it bears any form of life at all. Likewise, taking the earth as a whole, we find that large organisms occupy only a small fraction of the surface. However, we can reasonably expect to find evidence of microscopic life in any drop of water, pinch of soil, or gust of wind. Given a limited sample for study, microbiological analysis will certainly give the most reliable evidence for the presence of life anywhere on the planet. By the same odds the greatest diversity of biochemical mechanisms will be represented among the microbiota of a small sample.

Microbiological probes also offer distinct advantages for the collection and analysis of living material. From a single particle, microbes can easily be cultivated within the confines of an experimental device. In this they remain accessible to physiological and chemical experiments that would be extremely cumbersome with larger organisms. (Compare, for example, the automatic instrumentation that would be needed to catch a mouse or an elephant and then to determine its nutritional requirements!). The techniques of cytochemistry already developed for the chemical analysis of microscopic cells and organisms appear to be the most readily adaptable to automation and telemetric recording, an important advantage under the existing pressure of time, talent, and cost. Important issues of policy cannot be decisively settled without factual information on the growth capacity of the microorganisms that might be exchanged among the planets. Accordingly, methodological precedents in terrestrial science for exobiology are most evident in microbial biochemistry. The conceptual aims are equally close to those of biochemical genetics. Needless to say, no other resource or objective of serious biological science can be neglected in the development of an experimental program.

Aside from experimental designs, the pace of exobiological research may be regulated by advances in vehicular and guidance capabilities and data communication. In the expectation that these will remain in reasonable balance —for static or real time television communication with the planetary probe— the microscope may be the most efficient sensory instrument. The redundancy of a pictorial image would not be altogether wasted: would we put our trust in a one-bit pulse from an efficient black box to answer our queries about the cosmos?

According to this experimental concept, the terminal microscope-Vidicon chain must be supported by three types of development: (i) for collection and transport of the specimen to the aperture of the microscope; (ii) for cytochemical processing of the samples; (iii) for protection of the device against environmental hazards, for appropriate location after landing, and for illumination, focusing, and perhaps preliminary image selection. Detailed studies of these problems are only just under way, and the following suggestions are only tentative.

The easiest specimens to obtain may be atmospheric dust and samples of surface soil, once the device has been landed. These would be collected on a traveling ribbon of transparent tape which would be thrown out and then rewound into the device. Larger samples, collected by a soil auger, could be subjected to a preliminary concentration of nonmineral components by flotation in a dense liquid. The use of such a tape would simplify the problem of treating the samples with a succession of reagents—for example, specific enzymes and fluorescent stains for the detection of nucleic acids and proteins. Microscopy with ultraviolet light, particularly at 2600 and 2800 angstroms, owing to its selectivity for nucleic acids and proteins, may be the most direct way to distinguish microorganisms from mineral particles. Generally speaking, the microscope can be adapted to many simple analytical procedures whose construction on a larger scale would present formidable problems for automatic technique.

The adaptation of the microscope system to a payload can be undertaken more realistically when laboratory prototypes have been built and tested. For example, we will have to decide between accurate prefocusing of a microscope whose lenses and entry slit are mounted in a rigid structure and continuous control of focus by an optically controlled servo system (an innovation that would be far from useless in the biological laboratory). Fluorescent staining may facilitate automatic discrimination for conservation of radio power; the traveling ribbon can be stopped and the Vidicon-transmitter activated just when a stained object is in view.

These preliminary experiments can indicate some of the general features of the planetary microbiota. The data they furnish will support more intensive studies of the growth characteristics, chemical composition, and enzymatic capabilities of organisms cultivated on a larger scale. The interaction of these organisms with tissue cultures of animal cells can also be considered. From the results of these initial probes we can better deduce how to anticipate the long-range consequences of the intercourse of planetary biota.

Conservation of Natural Resources

A corollary of interplanetary communication is the artificial dissemination of terrestrial life to new habitats. History shows how the exploitation of newly found resources has enriched human experience; equally often we have seen great waste and needless misery follow from the thoughtless spread of disease and other ecological disturbances. The human species has a vital stake in the orderly, careful, and well-reasoned extension of the cosmic frontier; how we react to the adventuresome and perplexing challenges of space flight will be a crucial measure of the maturity of our national consciences and our concern for posterity.

The introduction of microbial life to a previously barren planet, or to one occupied by a less well-adapted form of life, could result in the explosive growth of the implant, with consequences of geochemical scope. With a generation time of 30 minutes and easy dissemination by winds and currents, common bacteria could occupy a nutrient medium the size of the earth in a few days or weeks, being limited only by the exhaustion of available nutrients. It follows that we must *rigorously* exclude terrestrial contaminants from our spacecraft. This stricture must hold until we have acquired the factual information from which we can assess with assurance the detrimental effects of free traffic and determine whether these are small enough to warrant the relaxation of these controls.

At the present time, the values that would most obviously be threatened by contamination are scientific ones. The overgrowth of terrestrial bacteria on

Mars would destroy an inestimably valuable opportunity for understanding our own living nature. Even if an intemperate mission has not contaminated a planet, the threat of its having done so will confuse later studies, if earth-like organisms are found. However, other values are also involved. Quite apart from strictly scientific concerns, would we not deplore a heedless intrusion on other life systems? It would be rash to predict too narrowly the ways in which undisturbed planetary surfaces, their indigenous organisms, or their molecular resources may ultimately serve human needs. If we have cause to prejudice these values, we surely would not wish to do so by inadvertence.

To guard effectively against contamination requires a nice appreciation of the ubiquity and durability of bacterial spores, which are well preserved in high vacua and at low temperatures and are rapidly destroyed only when kept at temperatures over 160°C. It is probable that spacecraft can be disinfected by the conscientious application of gaseous disinfectants, especially ethylene oxide, but this will succeed only if the procedure is carried out meticulously and with controlled tests of its effectiveness. Sealed components, if found to be potential sources of contamination, can be disinfected by chemicals prior to sealing, or subsequently by heat, or by irradiation at very high doses. The technology of disinfection is an expert one, and personnel already experienced in it should be delegated supervisory control.

The assessment of this problem involves a concept of risk that has not always been perceptively realized. The hazards of space flight itself, or of hard impact, or the planetary environment *might* suffice to neutralize any contaminants, but can we afford to rely on uncertain suppositions when the stakes are so high, and when we have practical means at hand for conservative protection? We must be especially sensitive to the extreme variations in the environments of spacecraft or of planetary surfaces which might furnish refuges for microbe survival no matter how hostile the *average* conditions.

The indication by agencies both in the United States and the U.S.S.R. that adequate precautions will be exercised on all relevant missions is an important step in the realization of constructive exobiology.

Scientists everywhere will call for the application of these measures with the same care and enthusiasm as the more positive, exciting, and patently rewarding aspects of space research. Scientific microbiology in the laboratory is absolutely dependent on the rigorous application of the special technique of pure culture with aseptic control. If we do not exercise the same rigor in space science, we might as well save ourselves the trouble of thinking about, and planning for, exobiological research.

While early traffic to the planets will be one-way, we must anticipate round-trip, and even manned, space flight. Undoubtedly, planetary samples can be analyzed for any scientific purpose more conveniently and more exactly in the terrestrial laboratory than by remote devices. For each step of analysis, special devices can be used (or if need be, newly designed and constructed), and a constant give-and-take between human judgment and instrumental datum is possible. However, the return of such samples to the earth exposes *us* to a hazard of contamination by foreign organisms. Since we are not yet quite certain of the existence of planetary (that is, Martian) organisms, and know nothing of their properties, it is extremely difficult to assess the risk of the event. The most dramatic hazard would be the introduction of a new disease, imperiling human health. What we know of the biology of infection makes this an extremely unlikely possibility; most disease-producing organisms must evolve very elaborate adaptations to enable them to resist the active defenses of the human body, to attack our cells, and to pass from one person to another. That a microorganism should have evolved such a capacity in the absence of experience with human hosts or similar organisms seems quite unlikely. However, a converse argument can also be made, that we have evolved our specific defenses against terrestrial bacteria and that we might be less capable of coping with organisms that lack the proteins and carbohydrates by which they could be recognized as foreign. Furthermore, a few diseases are already known (for example, psittacosis, botulism, aspergillosis) whose involvement of man seems to be a biological accident. These arguments can only be resolved by more explicit data. Nonetheless, if they are harmful at all, exobiota are more likely to be weeds than parasites, to act on our agriculture and the general comfort of our environment, and to be pervasive nuisances than acute aggressors. However, even the remotest risk of pandemic disease, and the greater likelihood of serious economic nuisance, must dictate a stringent embargo on the premature return of planetary samples, or of craft that might inadvertently carry them. Again, our preliminary experiments must give us the foundation of knowledge to cope with exoorganisms, even to select those which may be of economic benefit. A parallel development of techniques for disinfection may mitigate some of these problems; at present the prospects for treating a returning vehicle to neutralize any possible hazard are at best marginal by comparison with the immensity of the risks.

Of the possible payloads for interplanetary travel, living man, of course, excites the widest popular interest. In due course, he may be supported by a sufficient payload to accomplish useful tasks in exploration beyond the capacities of instrumentation. However, he is a teeming reservoir of microbial contamination, the most difficult of all payloads to neutralize, and he is an especially suitable vehicle for infectious organisms. In view of these difficulties, and insofar as manned space flight is predicated on the return of the crew, a sound basis of scientific knowledge from instrumented experiments is a *sine qua non* for the planning of such missions.

Timely effort now to devise and build instrumented experiments is essential to keep pace with the technical capacities of space vehicles.

Conclusion

Many of the ideas presented in this article are not new. In the scientific literature they have been treated only occasionally, for example in a remarkable article by J. B. S. Haldane (1954). They are also anticipated in the classic works of science fiction—for example, H. G. Wells' *War of the Worlds*—and in a flood of derivative fantasies of less certain quality either as science or as fiction. This kind of attention has not necessarily contributed to realistic evaluation of the biological aspects of space travel, discussion of which may still be dismissed as overimaginative by some of our colleagues. However, exobiology is no more fantastic than the realization of space travel itself, and we have a grave responsibility to explore its implications for science and for human welfare with our best scientific insights and knowledge.

Notes

The principles embodied in this article reflect the judgment of one among several of the scientific groups advisory to the Space Science Board of the U.S. National Academy of Sciences. However, they do not necessarily represent any official policy of the committed views of each consultant. The continued interest and advice of M. Calvin, R. Davies, N. Horowitz, S. E. Luria, A. G. Marr, D. Mazia, A. Novick, C. Sagan, G. Stent, H. C. Urey, C. B. van Niel, and H. Weaver, among many others, have been indispensable.

To document this article in detail with references to original sources would require a bibliography of inordinate length. Many of the issues are reviewed in the following works.

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Research Overhead and the Universities

Should government support of basic research in private universities cover overhead as well as direct costs?

Norman Kaplan

Should overhead, or the "indirect" costs of conducting basic research in the private universities, be fully reimbursed (*1*)? Contrary to the apparently increasing trend toward an affirmative answer among those in universities as well as in the federal government, this article examines the case for the other side (*2*). I would go even further and suggest stopping all overhead payments for basic research in private universities as soon as practicable.

The case for full reimbursement seems reasonably compelling. Its proponents cite the financial plight of our universities, their importance for the maintenance and extension of the nation's basic research effort, and the erosion of general funds from endowment and other such traditional sources. The universities, it is said, are too poor to be asked to continue sharing in the costs of conducting research, and the federal government should, in all fairness, assume the true burden for the research it seeks to support. Furthermore, research funds help the university to perform a task vital to our national interest by training our future scientists, so it follows that anything contributing to the financial stability of the universities contributes to our national welfare.

Although there is probably little disagreement with these propositions, it is the thesis of this article that concentration on them obscures some more basic problems and postpones the search for more general solutions. The plea for payment of full costs prematurely defines the problem, and so restricts our focus. It is a piecemeal approach to the larger problems of formulating a national science policy and of defining the role of the universities in that policy and in their own sphere as educational institutions. At best, the payment of full costs for basic research may partially alleviate the financial plight of the universities. But such action will not solve their financial problems. Nor will it solve the problems of research policies of the universities or the federal government.

A sharp reversal of the present trend toward full reimbursement (*3*) may force reconsideration of how basic research should be supported, where it can be done best, and how we can get the optimum return on the money invested in it. As a prelude to the consideration of such problems, this article focuses on the university's role as a producer of basic research in relation to its other goals. The claim for full costs is examined in the light of the university's fiscal policies on all its major activities, including research. Finally, some possible consequences of full reimbursement are discussed in the context of problems of university autonomy and control.

The University's Goals

There is probably general agreement that universities in the United States have as their primary goals the storing, transmission, and extension of knowledge (*4*). The means for reaching these goals are libraries, teaching, and research, and the "product" involved has something to do with knowledge. The university has not made a profit or typically realized full costs on its storage facilities, its teaching, or its research. The major client or customer, in the sense of both number and visibility, has typically been the student, and even he is not expected to pay the "full costs"

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of his education. Even the highest estimates rarely show the student contributing more than about half of the total costs incurred by the university in educating him. This deficit financing is typically accomplished through endowment income; from involuntary subsidies realized through the inadequacy of faculty salaries, which reduce the "true" costs; and through a variety of other means. The acknowledged fact is that the universities have not exacted "full costs" from their most visible and numerous clients in return for one of their most important products or services—the education of students (*5*).

True, some have argued that the student, or more precisely his parents, should contribute the full share of the costs of educating him. But it is probable that the vast majority of Americans are strongly opposed to the idea of an educational elite based on the individual's ability to pay full costs. And while the universities have straddled the issue by raising the tuition (but not nearly to the level where it would cover costs), they continue to favor the admission of students on the basis of criteria other than the ability to pay all costs. The universities have not threatened to "cut back" their teaching activities if they do not receive full payment from their customers; rather, they have sought to augment, from other sources, the funds available to make up the deficit. Teaching students has always been a "normal" activity of the universities; and within the context of our traditional American values and experiences, the universities seek supplementary income without "strings." Thus, the private universities generally tend to oppose government subsidies because of the traditional fear of government control.

The second of the three major activities of a university can be discussed briefly here. The storage of knowledge represents a relatively fixed cost, and a university has no choice but to try to maintain the best possible library as well as other facilities which enhance its standing as a repository of knowledge. Technical advances may bring about more efficient methods of storing information, in terms both of costs and of effective use by scholars. It is even conceivable, in these days of enthusiasm for strict financial accounting, that a system of charges will be worked out, for the use of these information-storage facilities, in which individual professor, student, departmental, research-project, and administration accounts will be charged in order to allocate the full and true costs.

The major point of the last of the three activities of a university—namely, the research activity—seems fairly obvious. If research has been, and continues to be, a normal activity of the university, why should there be any expectation of realizing "full costs" in this sector? Unlike the teaching sector, where it might be argued that the student expects a "return" regardless of how much money he has invested, the donor who provides money for basic research expects no return at all in the normal course of events except the usual acknowledgment in a footnote. However small the proportion of the total costs of a research project contributed by a research donor may be, his contribution might well be viewed as "manna from heaven." For every such contribution, large or small, reduces the total amount of money that the university has to spend from its own funds to support its normal research activities. Instead of accusing the foundations and government agencies of not shouldering their fair share and not contributing the "full costs," one could argue that the universities have not appreciated the actual financial contribution received and the related saving in time and effort otherwise spent in trying to raise these monies from other sources (*6*).

To ignore overhead and full costs for a moment, suppose that from the beginning of next year the government agencies, private foundations, and other research donors were to cease all payments to the universities in support of research. Since most of these research donors have not been paying the full costs, it might be expected that the universities would be forced to raise anew only a portion of the total research budget. How many universities would try to raise money for their general fund to maintain the present level of basic research activity on their campuses? Would university administrations be willing to support even a considerably contracted research program should the present research donors cease to make their present "partial" contributions?

If, as the universities have maintained, research is normally a central activity vital to a university's goals and functions in a society, then it is to be expected that each university will seek to maintain a certain level of research activity regardless of the financial details and arrangements. In fact, a reasonable operational test of what that level of research activity should be in a given university might well be the volume of research money which the university accepts willingly and gratefully when there is no contribution whatsoever to meet indirect costs. If the university argues that it has had to increase its administrative and financial staff or provide more heat and light as a result of the volume of research activity, then we have presumptive evidence that the university would not otherwise support that level of research activity and that it should consequently seek less financial assistance for research—to maintain a level of research activity closer to what it is prepared to support normally, much as it supports certain levels of teaching and student activity.

No one can answer the question with any degree of certainty, though it may be suspected on the basis of past performance alone that the university researchers might be forced to return to the days when there was considerably less enthusiasm for research on university campuses and even less support in the form of equipment, assistance, space, secretarial help, and even time (*7*), whereas in an analogous hypothetical situation involving teaching and the student, the university would spare no effort in trying to maintain current activity.

I wish to draw no conclusions from this exercise with hypothetical situations. The point is simply that many American universities have, by and large, paid only lip service to the importance of research. Not until the period of early small-scale stimulation by the private foundations and the more generous support by the government in the postwar period, did they provide much internal support for research.

Consequences and Questions for the Universities

Let us now assume that the research donors were to adopt a policy of full reimbursement for the indirect costs of conducting basic research in the universities. What consequences might we expect? What kinds of questions might then be raised by the universities, as well as by the federal government as the most important supporter of research?

In this article I allude only to some of

the more obvious questions and consequences. The first point is that full reimbursement of overhead costs will not solve the financial problems of American universities. But the specific desire to cover overhead costs of research could be satisfied if the present trend toward increasing support from the federal government is extended, since federal agencies now contribute a substantially greater proportion of full costs than other research donors.

The problem of overhead costs is undoubtedly of greatest importance to the relatively few universities and colleges now receiving the bulk of federal research support. Presumably, the present failure to reimburse full costs accounts in part for the absence of any extensive research effort at the vast majority of the nation's institutions of higher education. Full reimbursement may very well encourage many colleges and universities to expand their research activities beyond the level that their desires and capabilities warrant. Federal research support is already viewed as a "hidden" (and necessary) subsidy to higher education.

Concern over the real or apparent dangers of federal control over education has always been strong in American thought. With payment of full research costs, government concern over precise determination of full costs might very well become concern over the extent and nature of overhead expenses in universities. Further, academic freedom and, more precisely, the university's independence with respect to its own research program might be threatened by the centering of attention on the short-run problems of overhead costs.

I turn now to a somewhat more detailed discussion of these points.

While full reimbursement would undoubtedly ease one sector of the university budget, it would clearly not solve the over-all financial crisis in the universities. A recent National Science Foundation report shows that the universities had to provide about $30.2 million toward an estimated $72.7 million of indirect costs on a $327.5 million budget for research in 1957–1958 (*8*). There is no separate tabulation for the private universities, so the $30.2 million figure includes some unknown proportion provided by state and local governments—a fact disregarded in these figures. Thus, the maximum that might have been recovered from research donors in 1957–1958 to cover what the universities themselves estimated as their unreimbursed indirect costs on their research was $30.2 million. This figure would appear to be a very small portion of a university financial problem usually estimated in the $20- to $40-*billion* range (*9*).

Parenthetically, it may be noted that universities could alleviate their indirect-cost problem considerably by increasing the proportion of support received from the federal government. While federal support contributed just over 70 percent to the universities' total indirect-cost bill in 1957–1958, it also paid just over 80 percent of the total indirect costs reimbursed by any sponsor. The federal government reimbursed the universities for two-thirds of the indirect costs resulting from outside support, while all other sponsors only contributed one-third of the total indirect costs resulting from their grants to the universities (*8*, p. 7).

The concentration of funds in a relatively few universities (*8*, p. 6; *10*) has raised pointed questions about the role of such federal support to the universities. If these federal funds are a kind of *de facto* subsidy to education, then how can the concentration of support be justified (*11*)? Not only is it likely that there will be increasing demands for greater dispersal of federal support for research among institutions not now among the large beneficiaries but the criterion for federal support may continue to shift from research capability to the over-all well-being of the institution.

Administrative Control

Questions of "control" enter at a number of points. If the federal government were to pay the full costs of the research it sponsors, it might well be justified in raising questions about the universities' mode of operation and fiscal accounting policies. The government already does this through its auditing procedures, which are felt by many to be inappropriately complex and restrictive for the university setting. But it could go further and question both the extent of administrative costs and the particular methods that are used to allocate such costs to the research activity.

In the staff report of the Commission on Financing Higher Education, Millett points not only to the variability of such costs among institutions but also to the fact that the general average of such expenditures is relatively high. He notes that about "40 percent of total current operating expenditures of institutions of higher education as a whole falls within the three major reporting categories 'administration and general,' 'plant operation and maintenance,' and 'auxiliary enterprises' . . ." (*12*).

Millett continues: "the allocation of overhead costs has become so important . . . [because of] the very magnitude of the expenditures which are labeled 'administrative' . . ." (*12*). But such allocations are extremely complex in the framework of the universities' many functions and not merely in terms of any technical accounting problems. As President Lee A. DuBridge of the California Institute of Technology noted recently: "I do not know how to determine at Caltech how many pennies of each dollar go to teaching and how many to research. This is not because our bookkeeping department is inefficient, but only because we purposely mix the two activities so thoroughly that separate budgeting is impossible even in principle" (*13*, p. 111).

But such considerations may only be considered of minor importance in relation to the more serious implied threats to academic freedom noted by the Academic Freedom Committee of the American Civil Liberties Union in its statement of 24 November 1959. Here the Committee noted: "It must be clearly recognized that if outside financing of university research and graduate education, particularly in the natural sciences, continues to follow present patterns, it will inevitably lead to a very serious erosion of university control of university activities" (*14*).

The reaction to this report is described in a recent article in The New York *Times* by Fred M. Hechinger: "In The New York *Times* sampling of views of university leaders, most of the thirty-eight presidents who replied pointed to dangers on other campuses and a high safety record on their own. While they admit that outside patrons are paying much of the piper's wage, they insist that the academic tune, whoever is calling it, remains strong and sound" (*15*).

Moreover, many university leaders, like Lloyd V. Berkner, have long maintained that: "The dangers of Federal support are greatly lessened when the funds are administered by a variety of government agencies. The most dangerous thing that could happen would be a concentration of all Federal support in any one agency, be it the Department of Defense, the National

Science Foundation, or the Department of Health, Education and Welfare" (*16*).

But it must be noted that while we are still far from concentration of federal support, the latest available figures show that the three agencies mentioned by Berkner, plus the Atomic Energy Commission, accounted for 97 percent of the total federal expenditures for separately budgeted research and development in colleges and universities in fiscal 1958 (*8*, p. 5).

Concentration of all federal research support in a single agency would undoubtedly result in a more unified policy, which could lead to favoring particular institutions, scientists, or research approaches. And it would certainly decrease alternative support possibilities for projects turned down by that agency. But some observers already see the seeds of these dangers in the powers which could be exercised by the review panels, which now carry a major responsibility in advising the government agencies on the projects they should support. Thus, Lee A. DuBridge recently commented: "The chief threat of control has come not from the government agencies who administer the funds, but from the panels and advisory committees (composed largely of professors!) who pass upon projects and budgets before they are accepted. Many of these groups have steadfastly opposed proper overhead payments on research contracts, have opposed including allowances for the salaries of professors working on the projects, have opposed block or departmental grants, and have required of the prospective research worker such elaborate and detailed proposals and reports that a type of bureaucratic committee control has grown up which suppresses daring ideas and takes administrative control out of the hands of the universities themselves" (*13*, p. 113).

But keeping administrative control in the universities appears to be threatened from many other directions as well. In a recent statement by the President's Advisory Committee, a policy for educational and research growth in the universities was advocated which few, if any, universities accept, or are likely to accept, for the majority of their graduate faculties. The committee noted: ". . . graduate education cannot be effective in institutions which impose heavy burdens of routine instruction and administration on the research scholar who should have plenty of time free to work with his graduate students. *A single graduate course plus the supervision of research may be more than a full-time load for many a professor*" (italics mine) (*17*).

Above all, there is the danger that administrative control may slip away imperceptibly as a result of numerous small and seemingly insignificant decisions and developments. And it is this danger which is the very crux of my opposition to full reimbursement for the overhead costs of basic research in the universities. Seemingly, this is a relatively small problem, and full reimbursement might help some aspects of the universities' financial plight. This might very well be the immediate result, but in the long run it might be detrimental to the interests of both the universities and the federal government. As was noted in a recent New York *Times* article: "One danger [of outside research grants] is clear and present. It is as President A. Whitney Griswold of Yale University warns, that an institution's independence is 'given away or traded away a little bit at a time in individual bargains that seem necessary or profitable at the moment.' This is not necessarily a warning against outside grants; it is rather a warning against an aimless patchwork of sponsored activities" (*15*).

Summary

The question of overhead costs must be viewed in the broader perspective of a national science policy as well as long-range university goals. The probability is that the federal government will maintain and even extend its present level of research support. Whether the government will, or should, maintain a large number of disbursing agencies supporting research is certainly one crucial issue to be decided. Related to it, but at the same time important in its own right, is the question of how the money will be spent and, specifically, how that money will be used to influence the over-all structure of the national research establishment. Will the government seek to modify the present diverse structure of research producers in order to achieve greater possibilities of coordination, or will it seek to maintain the present diversity? If we are not to abdicate our responsibilities, both to the universities and to the national research structure, questions such as these must be faced in the context of an emerging national science policy (*18*).

References and Notes

1. It should be emphasized that I refer here only to universities and not to other types of research producers, such as industrial or independent laboratories. The discussion is restricted to support for *basic* research and, further, to the grant rather than the contract type of support. (The use of the research contract rather than the grant as an administrative convenience by some agencies presents some difficulties which need not be explored in this article.) By "indirect costs" I mean that sum of money (usually some percentage of anticipated costs for salaries, equipment, travel, and the like, specifically justified by the nature of the research proposed) which is added to the total requested by the research investigator.
2. For a most thoughtful and thorough study of the larger problems alluded to in this article, see C. V. Kidd, *American Universities and Federal Research* [Harvard Univ. (Belknap) Press, Cambridge, Mass., 1959]. The discussion in chapter 5 on the complexities of research costs, direct and indirect, is particularly relevant in the present context, although I disagree with Kidd's conclusions. Some of the other proponents of full reimbursement of indirect costs are quoted in the text. As another example of this general position, see the recent statement in "The Advancement of Medical Research and Education," *Final Report of the Secretary's Consultants on Medical Research and Education* (27 June 1959), p. 71.
3. The recent action of the National Science Foundation allowing 20 percent of total reimbursable direct costs for recoverable indirect costs is a case in point.
4. The American Council on Education statement on university goals and research policies is probably still the most relevant. See *Sponsored Research Policy of Colleges and Universities: A Report of the Committee on Institutional Research Policy* (Washington, D.C., 1954), especially p. 4.
5. The principle of covering costs in a university is not recognized for the educational function but only for what are termed "auxiliary enterprises," which include the operation of dining halls and dormitories, student union facilities, and intercollegiate athletics. As Millett points out, "all these activities have in common one and only one attribute—that in most colleges and universities the costs of current operation . . . are borne by direct-service charges [and] because the general expectation is that all or most of these particular activities of higher education institutions shall be self-supporting, they have been segregated for special treatment in the expense and income accounts" [J. D. Millett, *Financing Higher Education in the United States* (Columbia Univ. Press, New York, 1952), p. 101].
6. C. V. Kidd, *American Universities and Federal Research* [Harvard Univ. (Belknap) Press, Cambridge, Mass., 1959], p. 84.
7. Despite the growing recognition of the importance of research in universities, the relative recency of this recognition in America tends to be forgotten. See, for example, C. A. Elvehjem, "Basic research and the state university," in "Symposium on Basic Research," *Publ. Am. Assoc. Advance. Sci. No. 56* (1959), especially p. 88; also, A. H. Dupree, "Influence of the past: An interpretation of recent development in the context of 200 years of history," *Annals* 327, 19–26 (1960), especially pp. 24–25.
8. "Funds for Research and Development in Colleges and Universities, Fiscal Year 1958; A Preliminary Report," *Reviews of Data on Research and Development No. 19* (National Science Foundation, Washington, D.C., 1960).
9. These estimates of the total extent of the university financial crisis were reported in an article by F. M. Hechinger on the Arden House Conference in The New York *Times* (8 May, 1960).
10. The National Science Foundation survey collected data from 1900 independent and autonomous institutions of higher education in the United States. Only 363 institutions reported research and development operating expenditures, and 61 of these reported such small expenditures that they were excluded from this preliminary report. Of the 302 institutions included in the report, 71, or about 23 percent, accounted for almost 89 percent of

the total university research expenditures. The top 20 institutions accounted for 57 percent of the total budget.

11. For a recent expression of this concern, see F. M. Hechinger's report on the Arden House Conference, New York *Times* (8 May 1960).
12. J. D. Millett, *Financing Higher Education in the United States* (Columbia Univ. Press, New York, 1952), p. 177.
13. L. A. DuBridge, "Basic research and the private university," in "Symposium on Basic Research," *Publ. Am. Assoc. Advance. Sci. No. 56* (1959), p. 111.
14. "Statement Concerning the University and Contract Research by the Academic Freedom Committee of the American Civil Liberties Union," p. 2.
15. F. M. Hechinger, New York *Times* (24 Apr. 1960).
16. L. V. Berkner, *Phys. Today*, 7, 13 (1954).
17. "Education for the Age of Science" (statement by the President's Advisory Committee) (Washington, D.C., 1959), pp. 28–29.

18. The observations reported in this article grew out of a recent study of research organization in the U.S.S.R. and parts of Western Europe. This investigation was supported by a research grant (RG 5289) from the National Institutes of Health, U.S. Public Health Service.

Science in the News

Senate Space Committee Report Is Critical of NASA's Plans for Its New Office of Life Sciences

Last March the National Aeronautics and Space Administration organized its fifth major division, an Office of Life Sciences, whose function is to see that when the time comes to send a man into space the information necessary to keep him alive and healthy will be available. A major share of the necessary research is already being done in Defense Department laboratories, where the military services, for their own purposes, are doing a great deal of work with direct applications in the space program. The Army, for example, is doing work on minimum nutritional requirements, the Navy is investigating the effects on personnel of the restricted and isolated life in long-submerged submarines, and the Air Force is studying the physiological effects of extremely rapid accelerations. The NASA life sciences office is charged with keeping track of research in such space-related fields; it will sponsor research programs of its own where the required information is not already being sought in service and university laboratories whose projects overlap the interests of the space agency.

Space Committee Report

Last week Lyndon Johnson's Senate Space Committee issued a report, prepared by the committee staff, criticizing NASA for not doing enough to coordinate the activities of its life sciences office with the work being done in the service laboratories. The bulk of the 270-page report is given over to descriptions of 32 of the larger Defense research facilities doing work in space-related fields. The committee staff's comments occupy only a short introductory section. Here the report points out that the armed services will spend $38 million this year on space-related research, that 2800 civilian and military employees will have been involved in the work, and that the equipment and facilities at their disposal represent an investment of $61 million. The report contrasts the Defense programs with the $5 million first-year budget of NASA's life sciences office and its expectation of having 20 professional employees on its staff by June 1961, the end of the current fiscal year. The report states that NASA obviously should make full use of the service programs and that there isn't enough evidence that NASA is making satisfactory arrangements for doing so. Indeed, Senator Johnson, in a statement accompanying the report, went so far as to suggest that NASA might not even need a life sciences division if it made thorough enough use of the service programs.

Neither NASA nor the Republicans on the committee were very happy with the report. NASA is not anxious to get involved in a public quarrel with the congressional committee that supervises its activities, but the agency clearly felt that the criticism was at least premature. The life sciences office was barely four months old when the report was completed. It still has only nine professional staff members. At this stage NASA feels that the office has not had much time to demonstrate whether or not it is doing a good job of coordinating its research with the Defense laboratories.

Observers have interpreted the report as a case of seizing an opportunity to reflect the attitude of the majority of the committee, at least (perhaps encouraged by election-year fervor), that the Administration has not been doing a good enough job of coordinating the military and civilian space programs generally. For the minority members of the committee Senator Wiley (R-Wis.) reacted with a statement arguing that the place that needed better coordination was the Senate Space Committee. He said he hadn't heard anything about the report until he read about it in the newspapers.

Jury Decides Cigarettes Caused Lung Cancer But Company Is Not Liable

A jury in Miami, Florida, has decided that a man whose heirs were suing the American Tobacco Company had died of lung cancer, that the disease was caused by smoking Lucky Strike cigarettes, and that the American Tobacco Company could not be held financially liable for the man's death. (The man had smoked two to three packs of cigarettes a day for 30 years before the disease developed.)

A company spokesman interpreted the results as support for the position taken by its medical witnesses, who argued that there was no firm proof that cigarettes do cause cancer. A court official, though, interpreted the ruling as indicating that the jury felt that not enough had been known about the dangers of smoking prior to 1956, when the man's illness was diagnosed, to hold the company financially liable.

A lawyer for the tobacco company told the jury that a judgment for the plaintiffs would "sound the death knell of the industry." The terms of the jury's decision suggest that a future

case could easily be decided against a cigarette manufacturer, on the grounds that by that time a sufficient link between smoking and cancer *had* been established in the years prior to the man's death, and that the cigarette company was financially liable.

This would lead to a long series of appeals and an eventual decision by the Supreme Court. Meanwhile, the cigarette companies are spending several million dollars a year on research to produce a cigarette that will not leave its maker open to such disturbing charges.

NSF Grants for Research and Research Facilities

The National Science Foundation, which will receive a $25-million increase this year over its fiscal 1960 appropriations, will spend the bulk of the increase on grants for basic research in the program for improving research facilities. The NSF budget this year is $175 million. It expects to distribute about $67 million in research grants, an increase of $8.6 million over last year. It will spend $21 million on the research facilities program, with nearly all of the $6.4 million increase over last year going into the program, now in its second year, for general refurbishment of graduate laboratories.

Until last year the facilities program had been devoted almost entirely to paying for major pieces of equipment that institutions would have had difficulty in paying for themselves. But the announcement of the general refurbishment grants brought in requests for grants totalling some $50 million. Only $2 million was available last year. This year the grants will be increased to something over $8 million.

Another still smaller, but important, program is the $1.9 million in unrestricted grants to universities. This program, which allows the universities to control the money, rather than awarding it on a project basis from Washington, appears to command a great deal of enthusiasm within the Foundation, and, of course, within the universities. If the current small program works well and if opposition does not develop in Congress, it will probably be greatly expanded in the years ahead. The laboratory refurbishment program appears to be a step in the same direction, that is, of supplying the universities with money for a general strengthening of their science departments as opposed to supporting only specific projects and the purchase of specific major pieces of equipment.

The Foundation's budget breakdown also shows a decline in the Antarctic research program (from $6.2 to $5 million), a fairly modest increase in the scientific manpower program (from $64.5 to $67.3 million), and an increase in the information program (from $5.5 to $6.8 million). The manpower program covers fellowship grants, teacher training programs, and other efforts to produce more and better-trained scientists. The information program includes not only support of actual information gathering and distribution, such as the grant to the AAAS for a survey of Chinese science (see below), but also research in information systems: mechanical translation, electronic data processing, and the like.

AAAS Symposium on Science in Communist China

The National Science Foundation gave its formal approval last week to a $30,000 grant to finance the AAAS symposium on science in Communist China. The symposium will be held during the 1960 AAAS annual meeting beginning 26 December. The AAAS undertook the project after discussions among the foundation, the association, and a number of government agencies and scientific societies, and found there was wide-spread interest in the proposed survey, and sufficient material available to provide a satisfactory basis for study. The AAAS, because of its broad interests, is sponsoring the symposium, with more specialized societies cooperating. A number of government agencies will cooperate by supplying microfilm reproductions of Chinese journals in their files.

In recent months the Chinese have put restrictions on exports of their journals, but through 1959, at least, a large volume of material has been accumulated, including a number of ten-year surveys prepared by the Chinese in 1959 to demonstrate their growing strength in the sciences. The material has been divided into 23 subject fields, and a scientist familiar with the language, usually an American of Chinese ancestry, has been assigned to review the material in his area and to present a report at the symposium.

The symposium will be similar to one held by the AAAS in 1951 on Soviet science. One of the immediate uses the National Science Foundation expects will be made of the survey will be to use it as a basis for selecting the most valuable material to be translated in a projected program to make at least a small part of the Chinese output available to Western scientists.

Nuclear Conference To Open This Month

Latest results obtained with the world's newest and most powerful high-energy accelerators and new developments in theoretical high-energy and particle physics will be reported at the 10th annual International Conference on High Energy Physics, 25 August–1 September at the University of Rochester. Approximately 350 scientists from more than 30 countries —including nine Nobel laureates—will attend the conference.

Participants will include some 175 foreign delegates, the largest overseas contingent ever to attend the annual meeting. More than a dozen invited papers by Soviet bloc scientists will be presented.

For the first time, the International Atomic Energy Agency will send official representatives to the conference: Bronislaw Buras (of Poland) of the Agency's Division of Research and Laboratories, and J. Robert Oppenheimer, director of the Institute for Advanced Study, Princeton. The conference is expected to be the largest yet held, both because of the increased number of high-energy accelerators now in operation and because of the rapid advance of theoretical developments in high-energy physics.

During the sessions delegates will hear the first official reports on several of the high-energy accelerators that started functioning this year. These include the 25-Bev accelerator—at present the most powerful such machine—recently dedicated at the European Center for Nuclear Research in Geneva; the 3-Bev machine at Saclay, France; and the 1.5-Bev installation at Frascati, Italy. Conferees will also hear latest results on the 10-Bev machine at the Joint Institute for Nuclear Research at Dubna, U.S.S.R.

Founded at the University of Rochester in 1950, the conference was held in Rochester for its first 7 years. Since 1957, when it was placed under the auspices of the International Union of Pure and Applied Physics, it has been held in Geneva (1958) and Kiev (1959).

National Magnet Laboratory To Be Built at M.I.T.

The world's most powerful magnet will be part of a new research laboratory to be built at Massachusetts Institute of Technology under a $9,502,000 contract with the Air Research and Development Command. Created as a national research center, the M.I.T. Magnet Laboratory will produce high and continuous magnetic fields in the search for new knowledge about matter and energy. Groups from academic, government, and industrial research institutions in the United States, and visiting scientists from other nations, are expected to use the installation.

The laboratory will be able to produce a continuous magnetic field rated at 250,000 gauss. The strength of this field is more than 500,000 times larger than the earth's magnetic field. The laboratory's high magnetic fields will be tools with which to determine the properties and behavior of atoms, atomic particles, and aggregations of atoms in solid materials, as well as in liquids and gases.

Construction is expected to start about mid-1961, on a site in Cambridge that is adjacent to the M.I.T. nuclear reactor. The laboratory is expected to be in full operation by 1964 with an anticipated annual research budget of $2 million a year.

Francis Bitter, a pioneer in development of magnets and in the study of high field magnetic phenomena, is resigning his position as associate dean of science in order to assume primary responsibility for the design and construction of the new laboratory. He will also be chairman of the laboratory's policy committee and will assume new duties as professor of geophysics to initiate studies at M.I.T. of the magnetization of the sun and its planets and of the role of magnetic phenomena in the evolution of the solar system.

Five M.I.T. scientists will form the nucleus of the magnet research center.

Benjamin Lax, head of the division of solid state physics at Lincoln Laboratory, will be director of the new facility. Lax is directing the work of the group of scientists and students engaged in solid state research using high magnetic fields in M.I.T.'s present magnet laboratory.

Assistant director of the laboratory will be Donald T. Stevenson, group leader in solid state physics at Lincoln Laboratory. Lax and Stevenson will be responsible for assembling the laboratory's permanent staff and directing the scientific program.

Two other members of the initial group, Henry H. Kolm and D. Bruce Montgomery, have played leading roles in the more recent development of high field magnets and will contribute to the design of the new magnets, as well as to the over-all installation.

James M. West, now associate director of M.I.T.'s division of sponsored research, will become the assistant director for administration of the laboratory. Those scientists assigned to Lincoln Laboratory will continue to be associated with the M.I.T. Lexington (Mass.) facility.

Francis Bitter and Benjamin Lax.

Brookhaven's Synchrotron Begins Operation

The alternating gradient synchrotron (AGS) at Brookhaven National Laboratory, one of the world's two largest operating particle accelerators, ran for the first time on 29 July, producing a beam of protons at an energy of more than 30 Bev. This is the highest energy ever attained by a particle accelerator. Before reaching this energy, the AGS had run for about an hour at 24 Bev.

The completion of the AGS represents an important step forward in studies of nuclear forces and the properties of sub-nuclear particles. It will enable scientists in the United States to observe nuclear interactions at energies about five times greater than ever before possible.

Another accelerator, quite similar in design and size to the AGS, has been completed at the European Organization for Nuclear Research (CERN) Laboratory, near Geneva, Switzerland. The CERN proton synchrotron first produced a proton beam of 24 Bev last November, and has since been run at 28 Bev.

Since 1954, the most powerful accelerator in this country has been the 6.2-Bev installation at the University of California's Lawrence Radiation Laboratory. With a machine in the 30-Bev range, physicists at Brookhaven expect to learn more about the many kinds of particles, such as mesons and hyperons, and the various "anti-particles," that are produced in target nuclei by bombarding them with high-energy protons.

Construction of the AGS, which includes 240 magnets placed in a ring-shaped, 18-foot square tunnel ½ mile in circumference, was started in late 1955, following several years of preliminary design work. The total cost of this latest Atomic Energy Commission research facility, including a service and laboratory building and an experimental building, is about $31 million.

International Geological Congress Convenes

Geologists from all points of the compass will converge on the Nordic countries for the 21st International Geological Congress, which will convene in Copenhagen on 15 August. The congress will be in session until 25 August.

The International Geological Congress, the oldest international scientific congress in existence, was conceived and primarily founded by American geologists. The desirability of organizing an international geological congress was called to the attention of the American Association for the Advancement of Science in 1876 at its annual meeting in Buffalo, New York.

The general plan for organizing an international congress was approved, and the founding committee was appointed. The first International Geological Congress was held in Paris during the 1878 International Exposition. Its success was so clear-cut that it has continued to convene about once in 4 years ever since the original Paris meeting. Although it has no permanent organization and is dependent for its continuity on invitations received from delegates of some country during a meeting, it has functioned amazingly well.

The scientific sessions generally last about a week and are of great interest to most of the participants, but the field excursions that usually precede and follow the scientific sessions are as much of a drawing card as the scientific sessions themselves. These excursions have ranged in duration from 1 day to 3 weeks.

The host countries to this 21st International Geological Congress will be the five Nordic nations: Norway, Sweden, Iceland, Finland, and Denmark.

Standards Bureau To Build Facilities in Peru

Continuing 15 years of international scientific cooperation, the Instituto Geofisico de Huancayo, Peruvian counterpart of the U.S. National Bureau of Standards, has smoothed the way for NBS construction and operation of a very high-powered transmitter-receiver near Lima for space exploration by radar. Peruvian and United States officials have reached the final stages of negotiating a treaty and contract covering the use and operation of the mountain-valley installation. A special Congressional appropriation of $550,000 for the current fiscal year will finance construction and equipment.

On 1 August a four-man team from the Boulder Laboratories of the National Bureau of Standards arrived in Peru to begin work with the Instituto Geofisico on preparation of a site that is about 10 miles from Lima. The advance party consists of Kenneth L. Bowles, Gerard R. Ochs, Glen F. Miller, and John L. Green.

Ochs and Miller will remain in Peru to assist in directing the work of clearing and leveling the site and, as soon as this is complete, of putting up a prototype antenna whose features are to be incorporated in the final antenna design. They will also assist in supervising the construction of the building, collecting materials for constructing the equipment, and establishing a direct communications system with the Central Radio Propagation Laboratory at Boulder, Colo.

Tours of duty at the installation will be for several years, with a probable minimum of two. To foster international ties, the NBS staff and their families have had intensive instruction in Spanish. They will also avoid the traditional "foreign settlement" by renting private housing in Chaclacayo, a town on the fringe of Lima.

New Method of Space Exploration

Space-exploration by radar uses a new technique, developed by Bowles, for studying the ionosphere and exosphere with equipment located entirely on the ground. A preliminary experiment, conducted at Long Branch, Ill., from early 1958 through April 1960, gave satisfactory results out to 400 miles above the earth's surface. The method employs a transmitter with a peak pulse power of 6 megawatts which sends out from the earth a very high frequency wave, lasting from 50 to 150 microseconds, and an extremely sensitive broadside antenna of great area which detects the faint reradiation of the pulsed radio wave by free electrons in the upper atmosphere.

The new installation at the magnetic equator is expected to permit observations to be made at altitudes up to 1800 miles (3000 km), and to extend the capabilities of the technique to measure such characteristics of the atmosphere as ion temperature, percentage ion composition, and intensity of the earth's magnetic field. Accurate measurement of the last two of these quantities depends upon locating the radar within approximately 5 degrees latitude from the earth's magnetic equator, one reason for the choice of site. An additional feature of the new radar system will be the capability of observing the sun by means of direct radio reflections, with a sensitivity greater than that ever used before.

Science Foundation Announces Program of Institutional Grants

An experimental program of institutional grants was announced recently by the National Science Foundation. This program will be under the direction of a new Office of Institutional Programs, headed by Louis Levin. Levin has been deputy director of the foundation's Division of Biological and Medical Sciences.

The institutional grants are designed to strengthen the over-all scientific research and research-training effort of colleges and universities. The awards are intended to provide institutions with valuable flexibility for strengthening and balancing scientific research activities without specifying the particular activities to be undertaken with the funds. Institutional grants will complement foundation support of science and science education now provided through research grants for specific projects and through fellowships.

Amounts of grants allowable to an institution under the new program will be limited to 5 percent of foundation research grant payments made to the institution during the previous year, and in any case will not exceed $50,000 for any one fiscal year. During the first year of operation, institutional grants will be based on the 9-month period from 1 July 1960 through 31 March 1961. In succeeding years, they will be based on a full 12-month period, 1 April through 31 March. Institutions will be required to report to the Foundation each year on the use of the institutional grant funds.

The Office of Institutional Grants will also handle the foundation's program for the development of graduate research laboratories under which grants are made for modernization and expansion of research laboratories in United States universities.

News Briefs

Koch dismissal. The Illinois Division of the American Civil Liberties Union has assumed responsibility for litigation in the case of Leo F. Koch, who has been dismissed from his position as assistant professor of biology at the University of Illinois. Koch's contract was terminated a year early because of views on premarital sex relations he expressed in a letter to a student newspaper. Some 229 Illinois faculty members are reported to have publicly protested the university's dismissal action. The American Civil Liberties Union office at 19 S. LaSalle, Chicago 3, Ill., is receiving contributions to Koch's legal defense fund.

* * *

Science and society. Mount Allison University, Sackville, New Brunswick, is holding a Summer Institute on Science and Society, 12–14 August. Among the subjects to be discussed are "Science and culture in the coming society," "Prospects for the peaceful uses of atomic energy," "Nuclear weapons, public opinion, and the scientist," "Space and international relations," and "Science and the Cold War." Participants from the United States include Gerald Wendt, president, National Agency for International Publications, New York, and John P. Hagen, director, Office for the U.N. Conference on the Peaceful Uses of Outer Space, National Aeronautics and Space Administration.

* * *

Carbon-14 price drops. The Atomic Energy Commission has announced a reduction of 27 percent in the price of carbon-14, a radioisotope produced at the AEC's Oak Ridge National Laboratory. The price change, from $13 to $9.50 a millicurie effective 1 July, results from lower production costs. An earlier price reduction of 50 percent was effective on 1 June 1959.

* * *

Temperature. Under the sponsorship of the American Institute of Physics, a new Conference on Temperature, Its Measurement and Control in Science and Industry is being planned. Proceedings of an earlier such conference were published by the institute in 1940 and contained significant contributions from the fields of physiology and biology. The institute plans to hold a 3- to 4-day symposium in March 1961 in Columbus, Ohio, and James D. Hardy has been asked to be chairman of the program in physiology, biophysics, and medicine. It is hoped that this program will contain both contributed and invited papers, and it is also hoped that many biologists interested in temperature will find it possible to attend the meetings and to contribute to the discussion. The abstract deadline is *15 September 1960.*

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Radiological health unit. A department of radiological science, first of its kind, is to be established at Johns Hopkins University. A $2-million laboratory will be built for the new unit, which will operate within the School of Hygiene and Public Health.

Russell Morgan, specialist in radiological medicine and chairman of the Public Health Service's National Advisory Committee on Radiation, will head the department, which will be concerned with all aspects of radiological health. A major objective will be the training of health specialists and research workers for both national and international service.

* * *

AMA nurses unit. The American Medical Association has announced the formation of a new department of nursing to carry on liaison with national nursing organizations. Veronica L. Conley, who for the past 12 years has been secretary of AMA's Committee on Cosmetics, has been named director of the new department, which is part of the Division of Scientific Activities.

* * *

Harrison memorial volume. To honor the late Ross G. Harrison of Yale University, a special memorial volume of the *Journal of Experimental Zoology* will be published by the Wistar Institute Press. The volume, number 142, will be dated October 1959, the month after Professor Harrison's death. In addition to papers by associates and friends, the volume will contain a complete reprint of Professor Harrison's famous 1910 paper, "The outgrowth of the nerve fiber as a mode of protoplasmic movement." The memorial publication may be purchased separately.

* * *

Helminthological Society. The Helminthological Society of Washington will observe its 50th anniversary on 8 October. A scientific program will be held at the University of Maryland, and there will be a banquet at which AAAS president Chauncey D. Leake will be the principal speaker. For information, write to the Helminthological Society of Washington, Animal Disease and Parasite Research Branch, ARS, USDA, Beltsville, Md.

Scientists in the News

John B. Adams has been appointed to serve as director-general of the European Organization for Nuclear Research (CERN) until 1 August 1961. He has held the post in an acting capacity since the tragic death of Professor C. J. Bakker (Netherlands) on 23 April. However, beginning 1 October, Adams will be at CERN on a part-time basis. By agreement with the British authorities, he will divide his time between CERN and the United Kingdom's new laboratory for plasma physics at Culham, of which he is director designate.

Eugene P. Kennedy, professor of biological chemistry at the University of Chicago, has been named Hamilton Kuhn professor of the biological chemistry and head of the department of biological chemistry at the Harvard Medical School, effective 1 September. At present Kennedy is on leave as a senior fellow of the National Science Foundation at Oxford University, England.

Frank A. Geldard, chairman of the psychology department of the University of Virginia, has been named dean of the graduate school of arts and sciences. While serving as dean he will limit his teaching to one course in experimental psychology and will relinquish his duties as departmental chairman. He will continue his research studies of the human senses. Geldard is also head of the North Atlantic Treaty Organization advisory committee on defense psychology.

James W. Moyer, physicist, has been named research director of the Sperry Rand Research Center, which is to be built later this year in Sudbury, Mass. For the last 12 years Moyer has been associated with the General Electric Company in technical long-range planning and research posts involving advanced study in the fields of semiconductors, optics, spectroscopy, and physical chemistry. He has served also as a consultant to the National Bureau of Standards, contributing to the bureau's Free Radicals Research Project.

Jonas Carol, deputy director of the Division of Pharmaceutical Chemistry, U.S. Food and Drug Administration, has been selected to receive the 1960 Harvey W. Wiley Award of the Association of Official Agricultural Chem-

ists. The award is presented annually to a scientist who has made an outstanding contribution to the development of methods for the analysis of food, drugs, cosmetics, feeds, fertilizers, or pesticides, or for use in general analytical chemistry.

Paul D. Rosahn, associate clinical professor of pathology, Yale University School of Medicine, who has been serving as visiting professor of pathology at the University of Medical Sciences, Bangkok, Thailand, recently received an honorary degree from the Thai university at a special convocation in Bangkok. The degree was conferred by Prime Minister Sarit Thanarat acting as chancellor of the university. This is the first time that a foreigner has been so honored.

Robert C. Seamans, Jr., chief engineer of the Missile Electronics and Controls Division of the Radio Corporation of America, has been named associate administrator of the National Aeronautics and Space Administration, effective 1 September. He will succeed **Richard E. Horner**, who has resigned to become senior vice president, technical, at Northrop, Inc.

George K. Reid, formerly of the department of zoology and Bureau of Biological Research, Rutgers University, is now senior professor of biology at Florida Presbyterian College, a new 4-year, liberal arts college to be opened in September in St. Petersburg.

Recent major faculty appointments at Dartmouth Medical School are as follows.

Kurt Benirschke is professor of pathology and chairman of the department. Benirschke was formerly pathologist for the Boston Lying-In Hospital and associate in pathology at Harvard Medical School.

R. Clinton Fuller is professor of microbiology and chairman of the department. Fuller has been chief of the section of microbiology at Brookhaven Laboratories since 1954, and for the past 2 years has been a visiting scientist at Oxford University.

Lafayette Noda is professor of biochemistry and chairman of the department. Noda was formerly associate professor of biochemistry at Dartmouth. He succeeds **Manuel Morales**, who resigned the biochemistry chairmanship on 1 July to accept a career investigatorship with the American Heart Association.

John P. Bunker, now on the staff of the Massachusetts General Hospital and assistant clinical professor of anesthesia at Harvard Medical School, has been named professor and chief of anesthesia at Stanford University School of Medicine.

Also at Stanford, **Daniel J. Feldman** has been appointed director of rehabilitation services and associate professor of medicine. He is now associate professor at New York University School of Medicine in the Institute of Physical Medicine and Rehabilitation.

Frederick P. Ferguson, formerly professor of physiology and acting head of the department of physiology, University of Maryland School of Medicine at Baltimore, has been appointed chief of the research fellowships section of the Research Training Branch, Division of General Medical Sciences, National Institutes of Health.

Kanematsu Sugiura, member emeritus of the Sloan-Kettering Institute for Cancer Research, was awarded the Order of the Sacred Treasure, Third Class, by His Majesty the Emperor of Japan on 21 June in recognition of his contributions to Japanese-American friendship and of contributions in the field of cancer research.

F. J. Reithel, head of the chemistry department at the University of Oregon, Eugene, and currently on leave at the National Science Foundation, will be on sabbatical leave during 1960–61 at the National Heart Institute, Bethesda, Md. He will return to Oregon in the summer of 1961.

R. F. Rinehart has returned to his position of professor of mathematics at Case Institute of Technology after a 2-year leave of absence spent as director of special research and operations research at Duke University.

H. E. Butler, principal scientific officer, Royal Observatory, Edinburgh, Scotland, will be in this country until 16 September. His itinerary includes the Mount Wilson and Palomar Observatories, Pasadena, Calif.; Boulder, Colo.; Washington (2 Sept.); and New York.

George K. Davis, professor of nutrition in the University of Florida's College of Agriculture, has been named director of nuclear activities for the university.

Recent Deaths

Victor V. Anderson, Staatsburg, N.Y.; 81; psychiatrist and founder and director of the Anderson School, Staatsburg; first president of the American Orthopsychiatric Association; helped to establish the first child-guidance clinics in this country and aided mental health surveys in a number of cities and states; 26 July.

Duc Maurice de Broglie, Paris, France; 85; a member of the Scientific Committee of the French Atomic Energy Commission; a pioneer in nuclear research, including radioactivity, ionization of gases, and corpuscular physics; member of the Académie Française and the Royal Society of London; 14 July.

Sir **Ernest Rock Carling**, London, England; 83; noted surgeon and an authority on the effect of radiation on human beings; chairman of the International Commission on Radiological Protection; 15 July.

Ward F. Davidson, Port Washington, N.Y.; 69; retired director of research for the Consolidated Edison Company of New York; pioneer in the study of air pollution and smoke control; 12 July.

Stanley B. Freeborn, Woodland, Calif.; 68; chancellor emeritus of the University of California at Davis; taught entomology at Davis for 45 years; was an authority on malaria and malaria-bearing mosquitoes; 17 July.

Theodore F. Herdegen, Philadelphia, Pa.; chemist, documentalist, and administrator at Smith, Kline and French Laboratories; head of the documentation section in the company's science information department; 5 July.

Clyde K. M. Kluckhohn, Cambridge, Mass.; 55; professor of anthropology at Harvard University; played a major role in organizing the Russian Research Center at Harvard and directed it from 1947 to 1954; also served as curator of Southwestern American Ethnology at the Peabody Museum; author and coauthor of books on the physical, social, and archeological branches of anthropology; in 1949 his *Mirror for Man* won a $10,000 prize as the best book on science for the general public; 29 July.

Erratum: The name of the new chairman of Mathematics Division at the National Academy of Sciences—National Research Council is J. Barkley Rosser, not J. Barker Rosser, as given on page 341 of the 5 August issue.

Erratum: The 10th Pacific Science Congress of the Pacific Science Association will be held at the University of Hawaii, Honolulu, from 21 August to 6 September 1961, rather than 1960, as indicated in the list of Forthcoming Events in recent issues of *Science*.

Book Reviews

Conflict of Interest and Federal Service. Special Committee on the Federal Conflict of Interest Laws of the Association of the Bar of the City of New York. Harvard University Press, Cambridge, Mass., 1960. xvi + 336 pp. $5.50.

Two years of research by a distinguished committee of the Association of the Bar of the City of New York have resulted in a lucid explanation of the present conflict-of-interest laws; a penetrating evaluation of their effectiveness in operation; and a recommended program for improvement and clarification of the law in the light of modern conditions. A well-written book, the Bar Association study dramatically highlights the fact that our government is using archaic tools to deal with ethical problems—that these tools not only fail to achieve their purpose but, in addition, have pernicious side effects.

Before we examine this book in detail, it may be useful to briefly identify and isolate the problem it treats. The conflict-of-interest laws constitute a body of seven statutes, six of them criminal, designed to prevent venality, or the appearance thereof, in the decision-making processes of the federal government. Their purpose is to prevent situations of conflicting interest from arising, the interests in conflict being, on the one hand, the interest of the public in maintaining integrity in government and, on the other, the interest of the government official in his personal economic affairs.

At the outset, the authors present an analysis of the seven statutes in question. Their description is clear and concise, and should be easily followed by both lawyer and nonlawyer alike.

In one group are treated the four 19th-century statutes. Briefly, one statute forbids a government employee to assist, or act as the agent or attorney for, another in the prosecution of a claim against the United States (18 USC 283). A second forbids the government employee to render services to a private party in relation to any matter in which the United States is a party or is directly or indirectly interested (18 USC 281). A third generally prohibits bribery in connection with procurement of government contracts (18 USC 216). The final 19th-century law prohibits the government employee from acting for the government in a transaction with any business entity of which he is an officer or in which he has a pecuniary interest (18 USC 434).

In a second group are the two post-employment statutes. The first is apparently hortatory since it contains no sanction (5 USC 99). The second is, however, of greater concern, for it carries a criminal penalty if, during a period of two years after he leaves government service, a former employee acts as attorney or agent for the prosecution of a claim against the United States, provided that claim involves subject matter with which the former employee was directly connected while he was in government service (18 USC 284).

A final and perhaps the most troublesome of the conflict-of-interest laws forbids an employee to receive any salary in connection with his services as such officer or employee from any nongovernmental source (18 USC 1914).

The incisive analysis which accompanies each statute demonstrates the multitude of difficulties engendered by inartful wording and the fact that the laws provide solutions to problems of the 19th rather than the 20th century.

As the authors point out, the vague terminology in the statutes leads them to cut a wider swath than is either necessary or desirable: For example, what constitutes "transaction of business?" What is meant by "prosecuting a claim" or when is a person being paid "in connection with" services rendered to the government? This latter restriction, in particular, with its "buckshot" approach to conflicts of interest, is plainly ridiculous in an era of heavy governmental reliance on the services of part-time consultants and advisers. Can we expect successful scientists, professionals, and businessmen to give up vital economic benefits in order to work three or four days a month for the government? The answer is obvious, and where the Congress has not provided exemptions to this law, administrators have been forced to wink an eye and pass on to more pressing problems.

In contrast, by virtue of their narrow focus on 19th-century problems, the statutory restraints frequently fail to cover important areas of "substantive risk." For example, it is a paradox to speak only of "prosecuting claims" in an era when the government is a dispenser of television licenses, construction permits, certificates of convenience and necessity, and a multitude of other economic advantages. Similarly, it is incongruous to talk only of a transaction with a "business entity" when nonprofit educational institutions frequently have a greater stake in government contracts than their profit-seeking cousins in the market place.

The authors could undoubtedly list their indictment *ad infinitum* but the gravaman of their complaint is that our existing restraints are crude, obsolescent tools, unable to cope effectively with modern problems.

From a lawyer's standpoint, rendering advice on this body of law is a nightmarish endeavor. The authors accurately point out that "these statutes generate an array of legal devices considered to legitimately avoid the statutes." One may argue that no difficulty exists so long as lawyers can engage successfully in these mental gymnastics, but a serious moral problem arises when we place reliance on such efforts. There is no doubt, as the authors point out, that: "When this happens, the law is made to appear ludicrous, legal administration is undermined, the underlying policy of the law is subverted, and the conscientious carry the heaviest burden."

While this consideration alone may be sufficient to raise a hue and cry for a revamping of the conflicts statutes, the study carefully points out the additional unwholesome effect which the statutes have on recruitment, particularly recruitment of political executives and intermittent employees. But, in the special chapter devoted to problems of the scientist, all the critical factors involved in evaluating the con-

flict-of-interest laws come into sharpest focus.

The authors call attention to the fact that the federal government today pays for approximately one-half of all scientific research, engineering, and development in the United States and that scientific inquiry is the product of a joint venture between the public and private sectors of the economy. Scientific inquiry thus typifies the mixed economy in action, and it is precisely at this juncture that the lines between public and private interest become blurred.

The book records, with impact, the changed status of America's scientists. In the last decade, many scientists have become men of property with strong business or institutional ties to the private sector of the economy. At the same time, their government's need for intermittent advisory and consultative services has grown greater as the country finds itself challenged by an adversary that uses technological advancement as a Pied Piper's flute to lead the uncommitted and backward peoples of the world.

It is thus commonplace for the scientist who works as an intermittent government employee to have some substantial and personal economic interest residing with either a business firm or a university. The present system of restraints is simply unsuited to the problems faced by the men of science whose necessary posture is astride the public and private sectors of our economy. In one sense, coverage under the present system is too narrow; in another, it is dangerously broad.

There are many areas of substantive risk unreached by our present law because of its orientation toward representative services in the "prosecution of claims." The law is, in addition, virtually blind to the economic and prestige interests of our educational institutions. There is no question that substantial benefits can accrue to a university by having its key personnel sit on government boards which influence the direction of research and the placement of contracts.

On the other hand, as the authors note, "rigorous application of even the existing conflict of interest statutes could severely cripple governmental recruitment in a most critical area—scientific development." The authors are not alarmist in this warning, and I do not intend to be alarmist in echoing that conclusion, but it must be said that their observation rings true. Conscientious administrators operating under the requirements of this neolithic apparatus have been forced at times to do without the services of fine scientific talent. This problem will become severe in the future as the scientist becomes an increasingly significant figure in the public arena.

The scientist's case is but illustrative. Similar problems are faced by the lawyer or the businessman called upon to devote his talents to the government on a temporary or part-time basis. If we are to proceed under a system of law that fails to consider the growth of big government with its consuming need for professional and technical talent; the extensive development of a mixed economy; and the major increase in the use of consultants, intermittent employees, and contractor personnel; then we must be prepared for some decrease in the efficiency of governmental operation. Although Congress, in exercising its legislative and investigative powers, and administrative agencies, in promulgating regulations and codes of ethics, have done helpful, if uneven, work in handling conflicts problems, there remains an undeniable need for a sweeping revision of the law.

This study calls for a new approach to conflict-of-interest problems—a plan which "neither sacrifices integrity for opportunism nor drowns practical staffing needs in moralism." The authors have given us such a plan in the form of a well-considered program and implementing statute.

The new program suggested by the Association of the Bar of the City of New York is an integrated and comprehensive system of restraints applicable to employees of the executive branch. It combines that which is useful in our present body of law with a series of original proposals designed to balance the two principal policy considerations at stake—the preservation of integrity in government and the assurance that our country obtains the personnel it needs to progress and, indeed, to survive in the modern world.

Perhaps the most startling innovation in this new program is its resort to the administrative process rather than to the criminal law. The agencies of government would be responsible not only for enforcing the new program, but for amplifying it through the adoption of detailed administrative regulations. While the proposed statute sets forth six basic restraints on employee conduct, it will be for the agencies and the administration to spell out these restraints in greater detail, tailoring their regulations to their own particular needs. Here the authors have taken a cue from Aristotle: "It is impossible that all things should be precisely set down in writing; for enactments must be universal, but actions must be concerned with particulars. . . ." This observation is particularly applicable where the law seeks to deal with subtle ethical problems, for in no single embodiment of law can the multitude of conflict-of-interest problems be dealt with effectively. Flexibility is required, and all the tools to achieve that flexibility are found in the modern administrative process.

Although there is basic reliance on this administrative process, the authors have included a civil as well as a criminal penalty for flagrant violations. However, in the ordinary case, the agency will wield the more direct and practical sanctions of suspension, dismissal, or reversal of improperly influenced decisions. This intelligent proposal places authority where the responsibility is, or should have been.

Two other basic concepts are of interest. First, the program draws a vital and long-needed distinction between "regular" and "intermittent" employees of the federal government. The intermittent employee is subjected to fewer restrictions on conflicting interests: For example, there is no prohibition on his receipt of outside compensation. In addition, while a regular employee may not assist private parties for pay in transactions involving the government, the intermittent employee would be free to do so unless the transaction is, or had been, within two years, under his official responsibility or was a transaction in which he substantially and personally participated on behalf of the government.

A second important feature of the new program is the concept of government action. As defined in the statute, it reaches beyond the traditional "claim" or "contract," the integrity of which is so jealously and shortsightedly guarded by the present law. This new concept will cover transactions such as urban development loans, merger clearances, television franchises, and the multitude of privileges which can be dispensed by the modern executive branch.

After outlining these basic innovations in conflict-of-interest laws, the authors set out six basic rules of conduct for executive branch employees.

First, a government employee must

disqualify himself from participating in any transaction in the consequences of which he has a substantial economic interest. It is expected that the President, through regulations, will define "substantial economic interest." It should be noted also that disqualification may be required if the economic interest in question is held by the employee's wife, child, or firm. This rule thus recognizes qualitative and quantitative differences in economic interests. It also provides for exemptions to be made by the President in situations where an exemption is in the national interest.

A second restraint prohibits a regular employee from assisting others for pay in transactions involving the government. As noted earlier, this restraint is somewhat relaxed for the intermittent employee.

The authors further propose that regular employees will not be permitted to have their pay supplemented by "anything of economic value" from a nongovernmental source in consideration of services rendered to the government. This restraint would not be applicable to intermittent employees or those serving without compensation. It also excludes from the ambit of its coverage certain private security-oriented interests, such as industrial pension and retirement plans.

In two other sections, the authors delineate rules of conduct with respect to gifts and the abuse of government office. While the section on gifts adds little certainty to present law, the section devoted to abuse of office is new and of special interest. This latter provision forbids a government employee to use his office in such a manner as to induce a person doing business with, or subject to regulation by, the employee's agency to provide the government employee with "anything of economic value." This section thus effectively deals with bribery and the problem of blatantly improper gifts.

In the area of postemployment restrictions, we find another interesting change. Once having played a direct and integral part in a given transaction as a government employee, the individual, upon leaving government service, is thereafter permanently barred from assisting any other person in connection with that transaction. If the degree of contact amounts only to "official responsibility," the bar is for a period of two years. Thus, for situations of actual conflict, the authors properly suggest an indefinite bar, but for situations of potential conflict, they propose only a "cooling-off" period of two years. This section would be applicable equally to both regular and intermittent employees.

After a brief review of these new suggested restraints, it is apparent that they provide no panacea and that not all the vagary in the present law has been eliminated. But it should be remembered that the drafters anticipate that the President and his administration will fill in and amplify these proposals.

Unquestionably, the most significant aspect of this proposed program is its reliance on administrative procedures. In reality, the administrative agencies have always been responsible for regulating conflicts of interest, but now it is proposed that responsibility should be complemented by authority exercisable in accordance with clear, modern guidelines. To those who fear laxity in the administrative process, it can only be said that experience has proved the criminal law to be ineffective as an instrument for dealing with conflict-of-interest problems. A time for experimentation has arrived, and if that experiment takes the form suggested by this forthright and well-considered study by the Association of the Bar of the City of New York, I feel confident of its success.

JACK R. NEWMAN
Office of the General Counsel, U.S. Atomic Energy Commission

Clinical Chemical Pathology. C. H. Gray. Edward Arnold, London; Williams and Wilkins, Baltimore, Md., ed. 2, 1959. iii + 160 pp. Illus. $3.75.

The second edition of this short text preserves well the first edition's excellent presentation of the chemical aspects of disease. The chapters on the function of the liver have been modified to include the newly developed function tests, while the chapter on the chemical pathology of the alimentary tract has been much reduced in length. A new chapter on fats has been added, and the chapter on biochemical tests in endocrine disease has been rewritten to include recent advances. The discussion of salt and water deficiency is excellent.

While this book offers much to the medical student, it appears to be an essential companion for laboratory technicians; by using it, the latter will obtain an appreciation of the value and the limitations of biochemical analyses.

A. EDWARD A. HUDSON
Goldsboro, North Carolina

Quantum Chemistry. Methods and applications. R. Daudel, R. Lefebvre, and C. Moser. Interscience, New York, 1959. xiii + 572 pp. Illus. $14.50.

The appearance of this book fills a gap in the basic literature relating to the application of quantum mechanics to chemical problems. During the past 30 years, research in two areas related to this field has been extensive and varied: in the semiempirical quantum mechanical treatment of molecular properties and in the development of detailed and elaborate methods for obtaining more accurate wave functions for quantum mechanical methods used in studying molecules. Indeed, the literature is so vast that it discourages all but the most ambitious beginner who wishes to specialize in the field, and it presents a confusing mass of detail to the nonspecialist who wishes to use the results of these studies to understand his own problems. This volume provides an excellent summary of the situation to date and at the same time gives enough details concerning the various methods and their application to enable the interested chemist to use them in studying his problems.

Quantum Chemistry is divided into two parts. Part 1 proceeds from a relatively simple, nonmathematical presentation of the concepts of quantum mechanics to the development of the quite simple principles that are needed to understand the very numerous approximate methods developed for the study of bond lengths and angles, excitation energies and transition probabilities, reaction mechanisms and rates, and so forth. Each of these problems is then discussed in separate chapters, with copious examples for illustration. This part of the book is very easily read, and the discussions are sufficiently detailed to provide any interested chemist with the tools for a clearer understanding of the relationships between molecular structure and experimental results in either equilibrium or rate studies.

In Part 2 the authors delve much more deeply into quantum theory and discuss in detail the higher order approximations which are necessary once the useful limits of the simpler approximations are reached. This part of the book will be particularly useful to advanced graduate students who are planning to specialize in quantum chemical theory, and it will also be useful to the nonspecialist for checking his efforts

before he becomes too enthusiastic in applying the simple methods given in Part 1.

Most of both parts are devoted to approximations based on the molecular orbital approach, although other approaches, particularly the valence bond method, are discussed in some detail. As the authors admit, many of the methods of calculating wave functions and their properties have been omitted, but the extent of the actual coverage is impressive.

All in all, this book certainly represents the most useful and easily assimilated discussion of quantum chemistry and its applications in this field which is available to date. The authors are to be congratulated.

M. B. WALLENSTEIN
National Bureau of Standards, Washington, D.C.

Physics of the Atom. M. Russell Wehr and James A. Richards, Jr. Addison-Wesley, Reading, Mass., 1960. xi + 420 pp. Illus. $8.50.

Elementary Modern Physics. Richard T. Weidner and Robert L. Sells. Allyn and Bacon, Boston, Mass., 1960. xi + 513 pp. Illus. $8.50.

Here are two good, new books on modern physics, a field comprising chiefly relativity, quantum physics, solid-state physics, and atomic and nuclear structure. These books are written at about the same level—namely, to follow a general physics course in which calculus is employed. Both require calculus as a prerequisite; both use the rationalized mks system of units; both are about the proper length for a one-semester course; one contains 11 chapters, and the other, 12.

Both the textbooks have a gratifyingly large number of problems at the end of each chapter, and answers to odd-numbered problems at the back. In layout and typography they leave nothing to be desired; the many line drawings are clear, and a few halftone illustrations are included where most appropriate. Interestingly enough, in both books important equations are boxed for emphasis. In both, references for collateral reading, at a comparable or somewhat higher level, are listed at the ends of the chapters; the Wehr and Richards volume lists journal articles as well as standard reference or textbooks.

Wehr and Richards follow essentially the chronological sequence of the great discoveries in modern physics. The first three chapters give the atomic view of matter, electricity, and radiation; the fourth chapter presents the atomic models of Rutherford and Bohr. Chemical evidence is often quoted; this a useful feature, since most students at this level have studied general college chemistry. Some good experiments, not often seen in textbooks, are mentioned—for example, the experiment of Zartman and Ko, which confirmed the Maxwellian velocity distribution. The chapter on relativity is well done, and so is the one on x-rays. Then follow a chapter on waves and corpuscles, one on solid-state physics, and finally, four chapters on phases of nuclear physics.

The illustrations reveal excellent ingenuity and laudable clarity; the diagram of the x-ray powder-diffraction process is novel and most revealing (page 157). An item that will be found useful by instructors as well as by students is the 20-page appendix, listing in chronological order from 550 B.C. to 1958 the various key experiments and theories bearing on the atomic view of nature (the nationalities of the many persons included are also indicated). Another appendix lists the Nobel prize winners in physics and chemistry, through 1958.

The text by Weidner and Sells covers essentially the same subject matter, but it is organized in a nonchronological manner that is said to be logically coherent and sequential. The first chapter is an excellent review of the appropriate areas of classical physics. Next comes a chapter on relativity, then two on quantum effects, then three on optical and x-ray spectra. In the three chapters on nuclear physics, the subject is introduced by describing accelerating machines and detection devices; this is a rather novel order. The final chapter is devoted to molecular and solid-state physics. An attractive feature of this textbook is the short summary given at the end of each chapter. In fact, the subject matter, content, and level of the book can readily be ascertained by reading the 11 summaries (there is none for the first chapter).

A useful feature is the listing in large type, on the inside front and back covers of the book, of the nine physical constants and the four rest masses and rest energies commonly used in computations. Students working on problem assignments will find this to be a great time saver.

The choice between these textbooks will probably be made on the basis of whether or not the prospective user feels strongly about the chronological order of presentation, and whether his pet topics are emphasized. For example, Weidner and Sells give a more thorough treatment of atomic spectra, whereas Wehr and Richards illustrate the band theory of solids by explaining the operation of a number of semiconductor devices. We feel that each book is an excellent treatment of modern physics on the elementary level and that these textbooks constitute a formidable challenge for subsequent authors and publishers in this field.

S. S. BALLARD
F. E. DUNNAM
Department of Physics, University of Florida

The Ongoing State University. James L. Morrill. University of Minnesota Press, Minneapolis, 1960. 143 pp. $3.50.

Much has been said and written about the American state university, but never more wisely than in this book, *The Ongoing State University*, by James L. Morrill. Published just before the author retired after serving for 15 years as president of the University of Minnesota, it is a skillful adaptation from what must have been a considerable number of speeches made between 1945 and 1960.

The first three chapters—"The land-grant idea," "Knowledge for use," and "Servant of all the people"—highlight service aspects of the state university and set the tone for the volume. Developmental and administrative features of American higher education, which so frequently confuse and confound foreign observers, are dealt with in chapter 4. Five chapters are concerned with such prickly topics as academic freedom and responsibility, religion in the state university, athletics, public relations, and the alumni. Finally, to round out a dozen closely packed chapters, Morrill makes some pointed observations about the responsibility of a state to its university, the relations of higher education and the federal government, and education as an investment in the future.

In his preface, Morrill states: "Like politics, university administration is also the art of the possible, of helping to make possible the aims of both the philosophical and the practical as scholarship and science and society

are able to perceive and conceive these aims." Although some scholars and scientists regard the administrative role as largely supernumerary, I believe that all who have known the distinguished career of "Lew" Morrill will agree that his positive leadership in higher education has meant a great deal, not only to Minnesota but also to the nation as a whole. This book will be of particular interest to them. More than that, however, it is full of insights which will enhance anyone's understanding of an increasingly important American institution, the state university.

LOGAN WILSON

University of Texas, Austin

Introduction to Quantum Field Theory. F. Mandl. Interscience, New York, 1960. vii + 202 pp. Illus. $6.

Two claims are made for this slender volume: first, that it is an introduction to the basic ideas of field quantization and, second, that "it should make the reader expert at handling modern perturbation-theoretic methods in quantum theory." One might be inclined to question the optimism with which the second claim is phrased, but it must be admitted that the author has a knack for exposition that goes far to substantiate the claim.

The weakness of the book is all too obvious, and perhaps even inevitable. Quantum field theory is a subject which is not yet in a definitive form. Therefore one must certainly include a large amount of background material in order to make intelligible the more recent developments which are not included in the present volume. This background material is provided in the book written by Schweber, Bethe, and de Hoffman; and in essence, Mandl has included no more modern material in his volume than is included in the above mentioned text. There is no mention of the recent work of Jost, Lehmann, Symanczik, and others who have explored the results of dispersion theory. This is a regrettable omission, in view of the book's 1959 copyright date.

The topics discussed are presented with a notable felicity of style. Mandl begins by axiomatically introducing creation and annhilation operators, that is, by omitting the usual treatment of harmonic oscillators. This topic is followed by a discussion of classical fields, field quantization, and the interaction representation; and the results obtained are applied in some detail to mesons, fermions, and photons. Then the author introduces interactions and this discussion leads to a consideration of the S-matrix and its representation in terms of Feynman diagrams. The elementary results are applied to Compton and Coulomb scattering. Finally, some of the refined results of diagrammatic techniques are presented and renormalization is discussed.

It is difficult not to recommend this book; the style of writing is uniformly high, but the book is inadequate as a text for a course in quantum field theory because of its omissions. It is, nevertheless, a valuable introduction to diagram methods and can be recommended to workers in statistical mechanics and solid state physics who may not be as interested as theoretical physicists in the intricacies of quantum electrodynamics.

GEORGE WEISS

Institute for Fluid Dynamics and Applied Mathematics, University of Maryland

New Books

Biological and Medical Sciences

Marti-Ibanez, Felix, Ed. *Henry E. Sigerist on the History of Medicine.* M.D. Publications, New York, 1960. 324 pp. $6.75. Twenty-seven papers are reprinted from various journals and books.

McDonald, Lawson, Ed. *Pathogenesis and Treatment of Occlusive Arterial Disease.* Lippincott, Philadelphia, Pa., 1960. 249 pp. $5. Proceedings of a conference held in London at the Royal College of Physicians on 13–14 November 1959.

Newell, Frank W., Ed. *Glaucoma.* Josiah Macy, Jr. Foundation, New York, 1960. 224 pp. $8. Transactions of the 4th conference, 1959.

Peeters, H., Ed. *Protides of the Biological Fluids.* Elsevier, New York, 1960 (order from Van Nostrand, Princeton, N.J.). 430 pp. $15.75. Proceedings of the 7th colloquium held in Brûges; some of the papers presented at the colloquium have been published in *Clinica Chimica Acta*; only summaries of these papers are included in this volume.

Reynolds, S. R. M., and Benjamin W. Zweifach, Eds. *The Microcirculation.* Univ. of Illinois Press, Urbana, 1959. 178 pp. $4.50. Proceedings of the 5th conference on microcirculatory physiology and pathology; presents discussions on the factors that influence the exchange of substances across the capillary wall.

Roberts, D. F., and G. A. Harrison. *Natural Selection in Human Populations.* Pergamon, New York, 1959. 84 pp. $3. Papers given by invited speakers at the first full meeting of the newly constituted Society for the Study of Human Biology, 8 November 1958; contributors are L. S. Penrose, A. R. G. Owen, C. A. Clarke, P. M. Sheppard, T. Dobzhansky, and E. H. Ashton.

Roemer, Milton, I., Ed. *Henry E. Sigerist on the Sociology of Medicine.* M.D. Publications, New York, 1960. 407 pp. $6.95. Thirty-one essays, 28 of which have been previously published.

Semmes, Josephine, Sidney Weinstein, Lila Ghent, and Hans-Lukas Teuber. *Somatosensory Changes after Penetrating Brain Wounds in Man.* Harvard Univ. Press, Cambridge, Mass., 1960. 104 pp. $4.

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Reports

Pigmy Marmoset as an Experimental Animal

Abstract. The pigmy marmoset has been maintained under laboratory conditions for approximately 1½ years. A synthetic diet has been prepared, which includes milk, eggs, and lard, which may cause a fourfold increase in serum cholesterol levels. This species is proposed as an experimental subject for the study of aging and age-associated diseases.

It is desirable to determine the biochemical and physiological characteristics of every species. A particular trait in one may help to illuminate an area of investigation which is more complex or difficult to study in others. For example, for the study of aging and of age-associated disease, it would be desirable to locate a short-lived primate in which problems arise that are similar to those observed in man.

The pygmy marmoset, *Cebuella pygmaea* (also termed *Callithrix pygmaea*) (Fig. 1), is a New World monkey which grows to about 4 in. in length (exclusive of the tail) and a maximum weight of less than 200 gm. It comprises two subspecies, one of which, the Western (*C. pygmaea pygmaea*), is found in the forests of Equador, Colombia, and Peru; the other, the Eastern (*C. pygmaea niveiventris*), is found in Brazil (*1*). Although definite evidence is not yet available, reliable information suggests a longevity of 6 to 8 years (*2*). This is considerably less than the average life span of other primates for which adequate data are available (*3*). It thus has seemed worth while to investigate the possible use of this animal as an experimental subject for studying problems of aging. Our findings over a period of 1½ years are reported here.

Our pygmy marmosets, all of unknown age, were obtained from Iquitos, in eastern Peru; hence they are of the subspecies *niveiventris*. They were maintained in rabbit cages, each of which contained a small wooden box with an opening just large enough to contain all of the occupants of the larger cage, in relatively close proximity. As many as 20 pygmy marmosets were maintained in a cage, and they usually remained in the wooden box. This proved to be fortunate, for the cages could then be opened readily for cleaning and feeding without risk of the animals' escaping. Furthermore, when specimens were to be obtained, the box, with its occupants, could be put into a sack from which one marmoset was taken at a time. The boxes were not cleaned, since small primates do not do well in unfamiliar quarters; the presence of some of the excreta from their group is apparently necessary (*4*). A second box was introduced when it became necessary to remove the first, and the first was taken out when familiarity with the second had been acquired. In order to obtain specimens of blood, the marmosets were wrapped in a towel so that only the tail protruded. This seemed to tranquilize them. Microspecimens could be taken, as with rats.

It was essential that the temperature be kept uniform and high, and that the humidity should be high. A minimum temperature of 82°F was maintained with an automatic heater. The temperature rose above this with the heat of the summer. The air was maintained near water-saturation by means of a steamer.

Losses in newly acquired animals were due to infection accompanied by pulmonary diseases or diarrhea, or both. Losses of animals in the colony were due to the introduction of infected new arrivals which had not been properly quarantined, to fighting, to refusal to eat, and to accidental exposure to adverse environmental conditions.

Although the marmosets thrive on a diet of various fruits and meal worms, it was of first interest to determine whether they could be maintained on a diet more like man's, including milk, eggs, and lard. A liquid diet was prepared as follows: condensed milk, Carnation brand, 400 ml; corn syrup, Karo brand, 25 ml; water, 400 ml; four eggs, 175 ml; vitamin mixture, Vipenta No. 2, 1.2 ml; vitamin mixture (*5*); salt mixture (*6*).

It was calculated that 15 ml of this mixture, the average daily consumption, provided adequate nutritional intake. There was no other source of water. In addition, the marmosets were provided with Wheat Chex saturated with a mixture of lard and Karo syrup, each 50 percent by volume. This preparation was readily consumed. Though numerous attempts were made to introduce meat into the diet in various forms, it was not accepted.

It was necessary to wean the marmosets from the old, basal diet by presenting the new diet along with the basal diet and then gradually reducing the components of the latter one by one. Basal serum cholesterol levels (*7*) of new arrivals or of those on the basal fruit and meal-worm diet were 167 to 250 mg per 100 cm^3 (210 ± 28). Values were lower for young animals (those judged to be young by their smaller size). After introduction of the synthetic diet, the response of the animals' serum cholesterol was varied, apparently depending at first upon the degree of adaptation to the diet. If the animal did well on the diet, weight was maintained or increased and the serum

Fig. 1. The pygmy marmoset (*Cebuella pygmaea*).

Instructions for preparing reports. Begin the report with an abstract of from 45 to 55 words. The abstract should *not* repeat phrases employed in the title. It should work with the title to give the reader a summary of the results presented in the report proper.

Type manuscripts double-spaced and submit one ribbon copy and one carbon copy.

Limit the report proper to the equivalent of 1200 words. This space includes that occupied by illustrative material as well as by the references and notes.

Limit illustrative material to *one* 2-column figure (that is, a figure whose width equals two columns of text) or to *one* 2-column table or to *two* 1-column illustrations, which may consist of two figures or two tables or one of each.

For further details see "Suggestions to Contributors" [*Science* **125**, 16 (1957)].

cholesterol levels rose to a maximum value in 4 to 8 months. The greatest value obtained to date is 880 mg per 100 cm^3. In a number of animals, subsequent to the period of maximum level of serum cholesterol, which was maintained for several months, the value fell precipitously along with decrease in body weight. In many cases this was followed by death of the animals, in which general emaciation or evidence of pulmonary hemorrhagic edema, or both, was observed.

We are firmly convinced that this species will become a useful laboratory animal. To date there have been no successful matings, however. This may be due to the failure to separate pairs. We would appreciate any information which other investigators may have on the matter of propagation of this species (*8*).

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3. A. H. Schultz ["Postembryonic age changes," in *Handbuch der Primatologia*, H. Hofer *et al.*, Eds. (Karger, Basel, Switzerland, 1956), vol. 1, pp. 887–964], gives the following "rough averages of unpublished data by the writer, and data from reports of zoological gardens and other sources": lemur, 14-year life span; macaque, 24 years; gibbon, 30 years; orangutan, 30 years; chimpanzee, 35 years. Unfortunately, no adequate data for any New World monkey seem to be available; hence, whether the pygmy marmoset is typical of New World monkeys in general, or whether it is aberrant, cannot be said.
4. We are grateful to Dr. J. R. Hendrickson, University of Malaya, for supplying this information.
5. The vitamin mixture contained vitamin C, 250 mg; vitamin B_1, 4 mg; vitamin B_2, 5 mg; vitamin B_{12}, 20 mg; niacin, 20 mg; pantothenol, 20 mg; *p*-aminobenzoic acid, 20 mg; inositol, 1 gm; choline, 1.2 gm.
6. The salt mixture contained KCl, 24.4 gm; $FeSO_4 \cdot 7H_2O$, 13.6 gm; $MgSO_4 \cdot 7H_2O$, 14.4 gm; $CuCl_2 \cdot 2H_2O$, 0.42 gm; $ZnSO_4 \cdot 7H_2O$, 0.7 gm; $CoCl_2 \cdot 6H_2O$, 0.4 gm; $MnSO_4$, 0.2 gm; KI, 0.2 gm, in a volume of 125 ml.
7. The procedure of J. J. Carr and I. J. Drekter [*Clin. Chem.* **2**, 353 (1956)] was modified so that tests could be carried out on 0.025 ml of serum.
8. This work was supported by grants from the U.S. Atomic Energy Commission and the U.S. Public Health Service.

19 May 1960

Synxenic and Attempted Axenic Cultivation of Rotifers

Abstract. Three species of rotifers have now been grown synxenically and, to a limited extent, axenically. *Brachionus variabilis* thrives in suitable media containing *Chlorella pyrenoidosa* and a bacterial species. *Lecane inermis* and *Philodina acuticornis* var. *odiosa* are bacteriophagous, the former doing best with two bacterial species (dixenically), the latter doing well with *Escherichia coli* alone (monoxenically).

Relatively few species of invertebrate metazoa have been cultured synxenically [that is, such that a species is grown in the presence of a known number (one or more) of other species or of their living cells (*1*)]. Even fewer have been grown indefinitely under axenic conditions (*2*). During the past 2½ years several species of the aschelminth class Rotifera have been maintained in xenic culture (harboring a mixed, undetermined microbial flora) in our laboratory (*3, 4*). Certain of these have been established synxenically and numerous efforts to initiate axenic cultures have been made (*4–6*).

Our work has been done principally with two species of monogonont rotifers, *Brachionus variabilis* and *Lecane inermis* [the latter of which was erroneously referred to as a tiny, unidentified "bdelloid" in recent notes (*3, 6*)], and with a large bdelloid, *Philodina acuticornis* var. *odiosa* (*7*).

Xenic cultures of *B. variabilis* were first maintained in Petri dishes containing Seitz-filtered pond water, to which were added enough packed cells of axenic *Chlorella pyrenoidosa* (from a mineral medium) to give a semitransparent green turbidity apparent on inspection under the dissecting microscope. To initiate a culture in this medium one or more egg-bearing females, accompanied by a mixed microbial flora, were inoculated. More recently, *B. variabilis* has been grown well in 13-mm outside diameter, screw-capped test tubes containing the same basal medium except that the chlorellae are better derived from a mineral-glucose medium. *Lecane inermis* flourishes in Petri dishes of 0.1-percent Horlick's malted milk in distilled water. *Philodina acuticornis* grows vigorously in Petri dishes of distilled or pond water containing a ground-up Longlife infusoria table (*8*).

By various techniques the foregoing rotifers have been put into dixenic or monoxenic cultures with certain protists. This has generally been accomplished by first axenizing eggs with hypochlorite followed by antibiotic (penicillin and streptomycin) treatment (*6*) or by antibiotic treatment alone (*6, 9*). More detailed information is being published elsewhere (*4*).

Brachionids so far studied under synxenic conditions (*5, 8–10*) are typically algivorous. Until our culture became contaminated, we grew *B. variabilis* well dixenically for several months with serial subculturing in the presence of *C. pyrenoidosa* plus an unidentified species of gram-negative, rod-shaped bacteria. The basal medium was a 19:1 mixture of Seitz-filtered pond water and concentrated *C. pyrenoidosa* from a mineral-glucose medium. With *E. coli*, instead, as the sole bacterial species, the rotifers died out quickly.

By contrast, sustained monoxenic cultivation of *B. variabilis* has yet to be accomplished. Nathan and Laderman (*9*) have reported limited growth of this species in monoxenic association with *C. pyrenoidosa* in unsupplemented Seitz-filtered pond water; this has also been our finding. However, supplementation with traces of yeast extract and cyanocobalamine has led to flourishing cultures, although conditions permitting successful subculturing from these have not yet been worked out. Bazire (*10*) reported culturing *Epiphanes* [syn. *Hydatina*] *senta* monoxenically or dixenically, with one or two species of algal flagellates.

Our results with *Lecane inermis* and *Philodina acuticornis* indicate that both can live as bacterium-feeders.

Lecane inermis grows well in 0.05-percent Horlick's malted milk (in distilled water and Seitz-filtered pond water, 1:1) in dixenic association with *E. coli* and an unidentified species of gram-negative, rod-shaped bacteria, or with the latter plus a second unidentified species of similar morphology. With either of these unidentified species it also grows well monoxenically. It survives for a while, but does not reproduce in the company of *E. coli* alone. [After our recent report (*5*) of successfully growing *L. inermis* monoxenically with *E. coli*, we found that the second organism had been overlooked earlier because of its initially very slow growth in test media.]

Unlike *L. inermis*, *P. acuticornis* grows well in the presence of *E. coli* as the sole other living species. The best medium known at present is an empirically developed autoclaved extract of Longlife infusoria tablets.

Our efforts to culture rotifers axenically have so far met with very limited success, despite intensive effort.

The best axenic growth so far observed for *B. variabilis* has been in Seitz-filtered pond water to which were added low levels of whole human blood (about 1 percent), chick embryo ex-

tract (about 1 percent), yeast extract (about 0.001 percent), and cyanocobalamine (about 0.005 μg/ml (*5*); in this medium an immature female matured and produced an egg which did not hatch.

Lecane inermis has been tested in modifications of the liver medium used for the cultivation of axenic stocks of the nematode *Caenorhabditis briggsae* (*11*). In liver medium diluted to ¼ strength with autoclaved 0.1-percent malted-milk solution, lecanes have survived for at least a week and in a few cases have laid an egg, which has hatched. Various supplementations with vitamin mixes or glucose or both have not improved growth or survival. Our experience with *Philodina acuticornis* has been similar.

The rotifers we have so far worked with, whether algivorous brachioni or bacteriophagous lecanes and bdelloids, are clearly less tolerant of high levels of organic substances, and also of antibiotics (except possibly *P. acuticornis*), than are the rhabditid nematodes studied for several years in this laboratory (*11*). Nevertheless, it seems likely that, since *L. inermis and P. acuticornis* utilize gram-negative bacteria as food, they, at least, have nutritional patterns somewhat like such bacteriophagous nematodes as *Caenorhabditis briggsae*. When one or more rotifer species are tamed to axenic culture, it will be a logical next step to develop chemically defined [holidic (*2*)] media for their permanent maintenance. Like their aschelminth cousins, the nematodes, they promise to be useful tools in the study of comparative nutrition of the lower Metazoa and, most importantly perhaps, for fundamental studies in metazoan physiology and biochemistry (*12*).

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18 April 1960

Regulation of Reproductive Rate by Intra-uterine Mortality in the Deer Mouse

Abstract. Under crowded conditions (four mating pairs per cage), pregnant female deer mice of a partially domesticated strain, *Peromyscus maniculatus bairdii*, showed increased resorption of implanted embryos, and therefore a 60 percent reduction in effective natality, as compared with control females living at a population density of one pair per cage. No significant difference was found in the incidence of pregnancy or in the number of embryos implanted, but the adrenal glands of crowded females were enlarged by an average of 17 percent (by weight).

Population biology has become increasingly concerned with the study of fluctuations in the number of offspring produced by populations of animals. The problem of population cycles and crashes is related to the number of viable young produced by females in a population. Recent investigations (*1–3*) indicate that lower reproductive performance may be the result of stress induced by high population densities. The hypothesis that high densities create a stressed condition in members of such a population has been demonstrated by Chitty (*4*), Christian (*5–7*), Clarke (*8*), Louch (*2*), and others, while the effects of stress on endocrine responses have been extensively documented by Selye (*9*), comprising his "general adaptation syndrome."

Intra-uterine mortality (embryonic resorption) has been noted in many wild populations of mammals (*1, 10, 11*). It has been proposed that increased intra-uterine mortality may be the result of stress induced by high population densities and that it is a possible mechanism for the regulation of population size under natural conditions (*11*). Christian (*7*) demonstrated that crowding of albino mice resulted in adrenal hypertrophy, an increase of intra-uterine mortality, and disturbance of lactation.

In the light of such research the effects of crowding on reproductive performance were investigated in the JAX strain of the deer mouse, *Peromyscus maniculatus bairdii* (*12*). A pilot study was conducted to ascertain the feasibility of employing a widely used tranquilizer, reserpine, to vary stress levels in crowded mouse populations. Reserpine has a demonstrated ability to reduce aggressive behavior (*3*) and the incidence of fighting in crowded groups of mice (*5*). It was found, however, that effective dosages of reserpine caused significant increases in intra-uterine mortality and disruptions of the oestrus cycle, thus duplicating the postulated effects of stress on reproduction. As a result, reserpine studies on crowded populations were discontinued.

A standard procedure was used in all crowding experiments. Four male and four female *P. maniculatus bairdii* were placed in a 12- by 4- by 3-in. cage for 3 weeks. An excess of food and water was present at all times. A temperature of 74°F was maintained in the animal room and the room was constantly illuminated. Daily vaginal smears were taken of the females to ascertain the state of the oestrus cycle and the time of impregnation. Five such mating colonies were maintained. Additional mating pairs of the same stock were kept isolated in individual cages under the same conditions, as controls.

At the end of the 3-week period all female experimental and control animals were sacrificed. The animals were dissected, and the reproductive tracts were examined for resorbing and normally developing embryos and implantation scars. The mean paired weights of the adrenals of all experimental and control animals were calculated.

The paired adrenal weights of the crowded females averaged 5.12 ± 0.64 gm, while those of the isolated controls averaged 4.35 ± 0.14 gm. The increase of 17 percent in the crowded animals

Table 1. Reproductive performance of crowded and of isolated *Peromyscus*. Numbers given are means in each category.

Implantation sites (*N*)	Developing embryos (*N*)	Intra-uterine mortalities (*N*)
	Crowded animals (*20*)	
6.4	2.1 ± 0.96	4.3
	Control animals (*10*)	
8.3	5.2 ± 0.81	2.4

is significant at the 5 percent level. This agrees with findings of adrenal hyperactivity (*5–7, 13*) in crowded *Mus* populations and supports the conclusion that a stressed condition was induced in the crowded colonies.

The reproductive behavior of the crowded and isolated animals is summarized in Table 1. The results indicate that the crowded populations of deer mice show a decreased reproductive potential. Implantation scars showed that all but one of the crowded females became pregnant, while the number of scars, though somewhat smaller in the average member of this group, was not significantly reduced as it might have been under more extreme crowding.

Intra-uterine mortality was found in both the experimental and control animals, as indicated by the greater number of implantation scars than of developing embryos. There was, however, a much higher percentage of intra-uterine mortality in the crowded mice. The number of viable fetuses in the crowded group represents an effective reproduction rate only 40 percent of the rate for the uncrowded mice, and the difference (F ratio = 4.30) is significant at the 2 percent level of confidence.

It is therefore concluded that prenatal mortality is probably one of the means of lowering reproductive performance in dense populations of breeding mice. The inference that hormones of the adrenal cortex induce fetal resorption, though reasonable, requires further experimental proof, whereas ecologists will be interested in field confirmation of the suggestion that moderate crowding can act as a stressor in Selye's sense (*14*).

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17 May 1960

Studies on the Regulation of Fatty Acid and Cholesterol Synthesis in Avian Liver

Abstract. Homogenates of pigeon liver have been incubated with acetate-C^{14} in excess. Simultaneously, substrate for glycolysis was provided as glucose-6-phosphate. The incorporation of carbon-14 into cholesterol was maximal at low levels of glycolysis, whereas fatty acid turnover was maximum at higher glycolytic levels. A regulatory mechanism is proposed to explain the differential synthesis of cholesterol and fatty acids.

An interesting problem in the control of metabolic pathways is presented by the differential synthesis of fat and cholesterol under various pathologic conditions. For example, diabetic liver can synthesize cholesterol, but cannot synthesize fat normally. Supplementation with large amounts of glucose or fructose in vivo improves the rate of incorporation of acetate into fat by diabetic liver (*1*).

The carbon substrate for the synthesis of both fat and cholesterol is acetyl coenzyme A (acetyl CoA), and synthesis of both requires a hydrogen source. In each case the hydrogen source probably derives from the glycolytic pathway (*2, 3*).

A simple, but perhaps not altogether trivial, hypothesis for the selective control of the pathway of incorporation of acetate into lipide is derived from an analysis of the over-all balance of the carbon and hydrogen requirements for the utilization of equivalent amounts of acetyl CoA for cholesterol or fat synthesis.

$$18\ \text{Acetyl CoA} + 8\ \text{H} + \text{HOH} \longrightarrow \text{cholesterol} + 9\ \text{CO}_2 + 18\ \text{CoA}^{-\text{SH}}$$

$$18\ \text{Acetyl CoA} + 64\ \text{H} \longrightarrow 2\ \text{stearic acid} + 18\ \text{CoA}^{-\text{SH}} + 14\ \text{HOH}$$

This balance shows that the net molar hydrogen requirement for fat synthesis is about eightfold greater than the hydrogen requirement for cholesterol synthesis. It may be assumed that the diabetic animal in ketosis has a plethora of acetyl CoA, so that the controlling substrate factor might be the other requisite, the hydrogen source. Restoring glycolysis by the administration of glucose in vivo enhances the incorporation of acetate into fat in experimental animals (*1*) and relieves ketosis in the human diabetic (*4*). We therefore propose that the selections of pathways for utilization of acetate depend upon the total available reduced coenzmye, which is a function of the rate of glycolysis. To test this hypothesis, homogenates of pigeon liver were prepared and incubated with acetate-1-C^{14} in excess. Substrate for glycolysis was provided as glucose 6-phosphate.

Ten-percent homogenates of pigeon liver in 0.1M potassium phosphate (pH 6.5) were dialyzed against ten volumes of this buffer at 4°C for 1 hour to unmask a dependence of acetate incorporation on added glycolytic substrate. Longer dialysis resulted in an inactivation of acetate incorporation which could not be restored by the addition of nucleotides or coenzyme A. Preincubation with beta-amylase to destroy glycogen is not required. The dialyzed homogenate was supplemented with diphosphopyridine nucleotide (DPN), triphosphopyridine nucleotide (TPN) (*5*), Mg^{++}, Mn^{++} (*6*), coenzyme A, and acetate-1-C^{14} in excess. Two milliliters of this suspension were added to tubes containing glucose-6-phosphate and $KHCO_3$ (*7*), and the resulting solution was made to a final volume of 3.0 ml with 0.15M KCL and incubated with vigorous shaking for 1 hour at 37°C. Two milliliters of saturated KOH were added, and the mixture was saponified at 100°C for 2 hours. Mixtures of fatty acid and cholesterol were isolated according to the method of Hotta *et al.* (*8*) and counted for radioactivity.

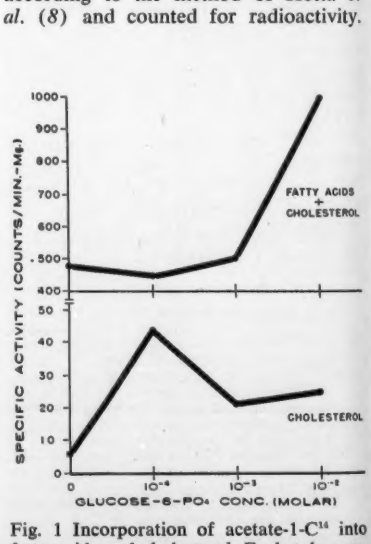

Fig. 1 Incorporation of acetate-1-C^{14} into fatty acids and cholesterol. Each tube contained $1.5 \times 10^{-4}M$ DPN, $1.5 \times 10^{-4}M$ TPN, $3.7 \times 10^{-5}M$ $MgSO_4$, $2.3 \times 10^{-4}M$ $MnCl_2$, $3.3 \times 10^{-5}M$ $KHCO_3$, 0.45 μg CoA, and $1.5 \times 10^{-5}M$ NaOAc-C^{14} (15 μc).

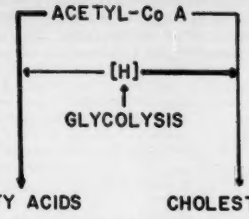

Fig. 2 Diagram of proposed regulatory mechanism to explain the differential synthesis of cholesterol and fatty acids.

Cholesterol was separated from the mixture as the digitonide (*9*) and counted. The resulting specific activities were corrected by zero time controls.

A typical experiment is shown in Fig. 1. Both cholesterol and fat incorporation were minimal in the dialyzed homogenate. Although incorporation into cholesterol is less than one-tenth of that into fatty acid, addition of small amounts of glycolytic substrate (glucose-6-phosphate) resulted in considerable enhancement of incorporation of C^{14} into cholesterol. As the concentration of glucose-6-phosphate was increased, C^{14} incorporation into total lipide increased, with concomitant diminution of incorporation into cholesterol. The availability of glycolytic substrate seemed to direct the anabolism of acetate to cholesterol or fat when the system was not limiting in acetate concentration.

The fact that the incorporation into cholesterol was optimal at low glucose-6-phosphate concentrations suggests a regulatory mechanism involving a limited supply of some component other than reducing equivalents. This is represented diagrammatically in Fig. 2. The data suggest that fat synthesis has a greater requirement for hydrogen equivalents than cholesterol synthesis. This is in agreement with the results of Siperstein and Fagan (*10*).

When acetyl CoA is generated at a constant rate it is mainly diverted toward fatty acid if an adequate hydrogen source (rapid glycolysis) is present, and only a relatively small amount finds its way into cholesterol. When, however, glycolysis is limiting, cholesterol synthesis is less affected than fatty acid synthesis, since it proceeds at a much slower rate and has a lower temporal as well as molal requirement for hydrogen. At certain levels of glycolysis (less than $10^{-4}M$ glucose-6-phosphate in Fig. 1) only fatty acid synthesis is diminished, and the decreased utilization of acetyl CoA by this system effectively diverts more acetate toward cholesterol, leading to the increased incorporation observed. Higher reducing potentials promote fatty acid synthesis which competitively withdraws acetyl CoA and thus decreased incorporation into cholesterol. Therefore, the net incorporation of acetate into total lipide is determined by the availability of both intermediate acetyl CoA and reduced coenzyme, whereas the partition of incorporation between fat and cholesterol is determined primarily by the availability of reducing potential.

The nature of the hydrogen source for fat synthesis has been elaborated by Green *et al.* (*11*), Langdon (*12*), and Siperstein and Fagan (*5, 10*), and appears to be a combination of the reduced forms of DPN and TPN. In our experiments glucose-6-phosphate furnished a source for reduction of both cofactors. The requirement of glycolysis for cholesterol synthesis has been demonstrated by Bucher and McGarrahan (*2*), but the exact cofactor requirement is still obscure. It appears from our experiments that the rate of glycolysis is a selective determinant for these two pathways, and it is suggested that the type of mechanism proposed above may warrant consideration as a general mode of differential biological regulation.

This interpretation suggests a close tie between nutrition and selective lipide synthesis. It is particularly interesting from the standpoint of the possibility that a localized enzymatic lesion in the arterial wall which results in diminished glycolysis might cause enhanced cholesterol synthesis and result in arterial cholesterosis (*13*).

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6 June 1960

Biological Activity of 3-Methoxy-Catecholamines

Abstract. The methoxy analogs of epinephrine, norepinephrine, and dopamine manifest some activity on smooth muscle and blood pressure. Metanephrine is as potent as epinephrine on rabbit aortic strips. The same compounds demonstrate a lesser potency on rabbit duodenum preparation, while 3-methoxy-dopamine produces a slight contraction of the duodenum. Both metanephrine and 3-methoxy-dopamine have 15 to 25 percent of the pressor activity of the original nonmethylated catecholamines. Normetanephrine is 1/600 as active as norepinephrine on the blood pressure of the cat.

The discovery of O-methyltransferase (*1*) has been followed by a reassessment of the role of amine oxidase in the biological inactivation of catecholamines. Recently, many authors have indicated (*2*) that substances which inhibit O-methyltransferase are also potentiators of catecholamines. However, there have been only a few preliminary experiments (*3*) to indicate that 3-methoxy-catecholamines are biologically inactive substances.

In the present experiment (*4*) the 3-methoxy derivatives of epinephrine, norepinephrine, and dopamine were tested in vitro on rabbit aorta spirals, according to the method of Furchgott and Bhadrakom (*5*), and on rabbit duodenum (*6*). The results are shown in Table 1, and the activities of the methoxy derivatives are compared with those of epinephrine. The same compounds were also tested for their effect on the blood pressure of the anesthetized, atropinized cat. These results also are summarized in Table 1.

All of the 3-methoxy derivatives are capable of contracting the aorta spirals at various degrees. These compounds were far less active on rabbit duodenum, where metanephrine and normetanephrine are, respectively, 1/40 and 1/200 as potent as epinephrine. On the contrary, 3-methoxy-dopamine behaves in this preparation as a mild cholinergic substance.

Both metanephrine and 3-methoxy-dopamine show an important activity on the blood pressure response. In comparing doses necessary for an equal response, these substances were found to be one-fourth to one-fifth as active as the nonmethylated compound. The case is different with normetanephrine, since this compound is only 1/600 as active as norepinephrine.

The experiments reported indicate that the methoxy derivatives of catecholamines are not entirely devoid of biological activity. It would seem logical to believe that O-methyltransferase is not the only enzyme involved in the

Table 1. Comparative activity of 3-methoxy-catecholamines.

Rabbit smooth muscle preparation			Blood pressure response in cat
Aortic strip		Duodenum	
Threshold ($\times 10^{-7}$)	Potency*	Potency†	Response of nonmethylated analog (%)‡
		Epinephrine	
0.1	1.	1.	100
		Metanephrine	
3.	1.18	0.025	25.
		Normetanephrine	
2.3	0.17	.005	0.16
		3-Methoxydopamine	
19.	.04	Contraction	15.

* Ratio between concentrations of epinephrine and methoxy-catecholamines required for production of 50 percent maximum contraction. † Ratio between concentrations of epinephrine and methoxy-catecholamines required to produce the same degree of inhibition. ‡ This percentage was obtained by comparing the doses required to obtain an equal rise of blood pressure with the methylated and nonmethylated catecholamines.

biological inactivation of catecholamines or that this enzyme is not equally effective for the inactivation of all catecholamines.

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25 March 1960

Influence of Uterine Site on Occurrence of Spontaneous Cleft Lip in Mice

Abstract. It has been found that within the A/Jax strain, embryos in the uterine site nearest the ovary develop cleft lip (with or without cleft palate) significantly more often than embryos in other positions in the uterus.

When anomalies occur in an inbred strain of mice, why do they occur only in certain members of the litter and not in others? Litter mates in an inbred strain have very similar, if not identical, genetic constitutions and, furthermore, they are all subject to the same external and maternal environment. The differences that determine that one embryo is abnormal and that others are not must reside within the uterus, and since the differences are not obvious, they must be small, although probably not simple. This report is concerned with one such difference—namely, that of a difference in the sites occupied by embryos within the uterus and the influence of this difference on the occurrence of cleft lip in the highly inbred A/Jax strain of mice. Cleft lip with or without cleft palate has been reported as occurring with a frequency of 6.33 percent in A sublines related to the A/Jax strain (*1*).

The uterus was removed from each of 76 A/Jax females 14 or 15 days after insemination and, after fixation, the positions of embryos with spontaneous cleft lip were noted. A total of 485 embryos from uterine horns containing two or more embryos had an over-all frequency of cleft lip of 8.5 percent. There were no differences in frequency between right and left horns. The frequency of cleft lip at the ovarian site (17/126 = 13.5 percent) is significantly higher ($p < .02$) than the total at all other positions (24/359 = 6.7 percent). This tendency for cleft lip to occur more often in the embryo occupying the site nearest the ovary appears even more clearly when only those horns (35 horns in 29 litters) that contain at least one affected embryo are considered. Again the proportion of embryos with cleft lip is significantly higher ($p < .01$) for embryos nearest the ovary (17/35) than for embryos at other positions (24/116) in the uterine horn. In Table 1 the data are arranged according to the number of embryos in a horn. It can be seen that there is a tendency for embryos with cleft lip to occur more frequently at the site nearest the ovary regardless of the number of embryos within a horn, but that this tendency appears to be greatest when there are either few or many embryos in the horn.

Further support for the foregoing observations was obtained from another group of A/Jax mice primarily intended for another purpose. In this group 48 litters were collected, just before term, from mothers that had received cortisone 11, 12, and 13 days after insemination. In this group of cortisone-treated fetuses (see Table 1) the tendency for cleft lip to occur more often at the ovarian site is again seen, but the difference in over-all frequencies (8/76 versus 8/161) is not significant. However, when only horns containing at least one affected embryo are considered, there are significantly more embryos with cleft lip (8/15) at the ovarian site than at the other sites (8/35); p, determined by Fisher's exact method, equals .03.

Table 1. Number and percentage (in parentheses) of cleft-lip (CL) embryos at the site nearest the ovary and at other uterine sites; the data are grouped according to the number of embryos per horn (horns containing one embryo are not included).

Embryos in horn (No.)	CL embryos (No. of total)			
	Ovarian site		Other sites	
	Untreated group			
2	4/26	(15.4)	1/25	(4)
3	5/32	(15.6)	2/64	(3.1)
4	2/31	(6.5)	5/96	(5.2)
5	3/22	(13.6)	6/89	(6.7)
6	1/8		3/40	
7	2/4	(20.0)	4/24	(11.8)
8	0/3		3/21	
Totals	17/126	(13.5)	24/359	(6.7)
	Cortisone-treated group			
2	4/31	(13.0)	1/31	(3.2)
3	1/18	(5.6)	2/36	(5.6)
4	2/16	(9.4)	4/48	(8.3)
5	0/9		1/36	
6	1/2	(9.1)	0/10	(2.2)
Totals	8/76	(10.6)	8/161	(5.0)

The data show clearly that cleft lip is more likely to occur in an A/Jax embryo that occupies the uterine site next to the ovary than in an embryo at any other site. At present it is not known what there is about this part of the uterus that makes it less favorable to normal development than the other parts. Perhaps there is a reduced blood supply which tips the balance, in a developmentally unstable embryo, in favor of abnormal rather than normal development. Since the ovarian site appears to be less favorable whether there are many or few embryos in the horn, the difference is more likely to be an inherent quality of that area of the uterus than an effect of other embryos in the horn competing for nutrients. The idea that this part of the mouse uterus is less favorable to the normal development of the embryo is supported by the work of Hashima (*2*), who found that embryos near term occupying the site nearest the ovary weighed significantly less than those at other uterine sites. It has also been noted by McLaren (*3*) that there is a tendency for "runts" in the TO random-bred strain of mice to occur significantly more often at the ovarian site than at other sites in the uterus. Gross examination of the blood supply, in vivo india-ink injections, and attempts to reduce the blood supply to individual embryos or to the entire uterine horn have not yet led to any significant findings (*4*).

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4. This work was supported by grants-in-aid from the National Research Council of Canada and the National Foundation, which are gratefully acknowledged.

9 May 1960

"Second Emerson Effect" in the Hill Reaction of Chlorella Cells with Quinone as Oxidant

Abstract. The second Emerson effect can be observed not only in photosynthesis but also in the photoreduction of quinone by *Chlorella* cells. This shows that this effect is not due to respiration and is not associated with the carbon dioxide–reducing phase of photosynthesis. Peaks at 650 and 670 mμ in the action spectra of this effect, in the case of quinone reductions as well as in that of photosynthesis in *Chlorella*, show that light absorbed by chlorophyll *b* as well as that absorbed by chlorophyll *a* 670 can effectively supplement far-red light (> 680 mμ) in both reactions; light absorbed by chlorophyll *a* 680 cannot do so.

The enhancement of the quantum yield of photosynthesis of *Chlorella pyrenoidosa* Chick strain 3 in the far-red region (> 680 mμ) by light of shorter wavelengths was first reported by Emerson and his co-workers (*1*) and was confirmed by Myers and French (*2*). However, there existed no direct evidence that this "second Emerson effect" (so called to distinguish it from the carbon dioxide burst at the beginning of a light period, often designated as the Emerson effect) was not due, at least in part, to light inhibition of respiration rather than to the enhancement of photosynthesis, because the techniques employed (manometry and polarography) could not distinguish positive changes in the rate of photosynthesis from negative changes in the rate of respiration. There was also no evidence as to the localization of the effect, either in the oxygen evolution phase or in the carbon dioxide reduction phase of photosynthesis. It was thought that study of the Hill reaction in quinone-poisoned *Chlorella* cells should provide answers to these two questions.

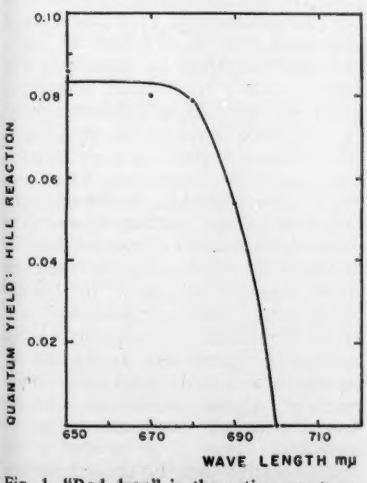

Fig. 1. "Red drop" in the action spectrum of the Hill reaction (photoreduction of quinone) in *Chlorella* cells. Quantum yield is plotted against wavelength of monochromatic light.

Para-benzoquinone (0.2 ml of a solution of 12.5 mg of recrystallized quinone in 5 ml of 0.01*N* sulfuric acid) was added to a thin suspension of *Chlorella* cells. Respiration, as well as capacity for carbon dioxide fixation and reduction, was completely inhibited by the addition of this solution. Nitrogen (99.9 percent N_2) was conducted through the manometer vessel filled with the suspension, for 15 minutes. Manometric measurements of oxygen liberation were then carried out at 10°C. Under these conditions the Hill activity lasted 4 to 5 hours.

Figure 1 shows the "red drop" of the quantum yield of oxygen liberation from quinone solution. The curve strikingly resembles that found for photosynthesis by Emerson *et al.* (*3*).

Figure 2 represents the action spectrum of the second Emerson effect in the quinone-*Chlorella* system. The results were obtained as follows: far-red light was applied to the suspension by placing Schott glass filters (RG8 and RG5, which give a sharp cutoff at 680 mμ) in the beam from an incandescent lamp. The rate of the Hill reaction in this light alone was taken as 100; the rate attributable to this light in the presence of additional shorter-wave light (between 640 and 690 mμ), obtained from the Emerson-Lewis grating monochromator (slit width, 5 mμ) was expressed as a percentage of that in the far-red light alone. In Fig. 2 the results are plotted against the wavelength of the supplementary light.

Govindjee and Rabinowitch (*4*) have demonstrated the existence of (at least) two forms of chlorophyll *a* in *Chlorella*, with different photochemical functions, with absorption peaks around 670 and 680 mμ, respectively. Light absorbed by the first form, like light absorbed by the accessory pigments, can bring about the second Emerson effect; this is revealed by the action spectrum of the second Emerson effect in the photosynthesis of *Chlorella*, which shows two

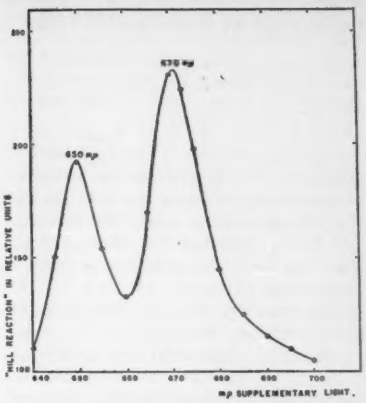

Fig. 2. Action spectrum of the second Emerson effect in the Hill reaction of *Chlorella*. Yield without supplementary light is designated as 100.

peaks in the red region—at 650 mμ (chlorophyll *b*) and at 670 mμ.

In the action spectrum of the second Emerson effect in quinone reduction we find the same two peaks (at 650 and 670 mμ, respectively) as in photosynthesis. This means that in the photoreduction of quinone by *Chlorella* cells, as well as in photosynthesis, the two types of chlorophyll *a* (chlorophyll *a* 670 and chlorophyll *a* 680) play different photochemical roles and that light energy absorbed by chlorophyll *b* probably is transferred to chlorophyll *a* 670.

The complete analogy between photosynthesis and the Hill reaction suggests that the second Emerson effect is not due to the inhibition of respiration and is not associated with the carbon dioxide-reducing mechanism of photosynthesis but with the oxygen-liberating mechanism common to photosynthesis and the Hill reaction (*5*).

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7 June 1960

Inhibition of Photosynthesis in Some Algae by Extreme-Red Light

Abstract. Photosynthesis produced by far-red light (about 700 mμ) is reversibly inhibited in some algae by extreme-red light (~ 750 mμ).

In earlier communications from this laboratory (*1*) it was reported that illumination of a suspension of the red alga *Porphyridium cruentum* with far-red light, obtained by filtering light from an incandescent lamp through a very dense aqueous solution of the phycobilins, produces little or no photosynthesis. Yet this light contains wavelengths above 660 mμ, a marked fraction of which are absorbed by chlorophyll *a*. (The absorption band of this pigment extends in vivo to about 720 mμ.) If bands 10 mμ wide obtained with a monochromator are used for illumination, measurable rates of photosynthesis are obtained at all wavelengths up to 700 mμ. Furthermore, an "Emerson effect" can be observed in phycobilin-filtered light: if orange light is added to it, the resulting rate of photosynthesis is higher than in the orange light alone. This confirms the observation that chlorophyll *a* is markedly excited by the phycobilin-filtered light.

These seemingly conflicting results can be explained by the assumption that the band obtained by the use of the phycobilin filter contains extreme-red (or near-infrared) light (> 700 mμ), which inhibits photosynthesis caused by far-red light.

In order to check this hypothesis, algal suspensions were illuminated with a far-red band isolated from the light of an incandescent lamp by a Farrand interference filter No. 1322, which has a transmission peak at 700 mμ but transmits some light down to 680 mμ. We made sure that the intensity of this light was far below that which causes saturation of photosynthesis. After photosynthesis in this far-red light had been measured, extreme-red light, with a band width of about 10 mμ centering at 750 mμ, was added and photosynthesis was measured again. The results of these experiments are shown in Table 1.

Table 1. Inhibitory effect of extreme-red light (750 ± 10 mμ) on photosynthesis of *Porphyridium cruentum* in far-red light (~700 mμ).

Experiment No.	Rate of photosynthesis (μl of O_2 per hour)		Rate in combined light (in % of rate in far-red light alone)
	In far-red light	In far-red light plus extreme-red band	
1	4.01	2.23	56
2	1.87	0.29	15
3	1.83	0.29	16
4	3.30	2.04	62
5	1.08	0.84	78

The scattering of the results in column 4 of Table 1 may be due to small unintentional variations in the culturing of the cells. Despite this scattering, it is evident that the rate of photosynthesis in far-red light is consistently and significantly reduced by the addition of light of 750 mμ wavelength. This effect is completely reversible.

Porphyridium cruentum must thus contain a pigment which absorbs in the extreme-red at about 750 mμ and which, when excited, inhibits the photosynthesis caused by far-red light. This pigment must be present in small amounts, since no band is recognizable in the absorption spectrum of *Porphyridium* in the region of 750 mμ.

Preliminary results indicate that a similar effect occurs in the green alga *Chlorella* but apparently not in the blue-green alga *Anacystis*. It remains to be seen whether it occurs also in higher plants (*2*).

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26 May 1960

An Interphylum Luciferin-Luciferase Reaction

Abstract. The light-emitting enzyme-substance systems, heretofore separated from different types of luminescent organisms, exhibit a marked biological specificity and comprise chemically different components. Extracts from a luminescent fish are now found to cross react with a crustacean system; some properties of the new system and implications of the phenomenon are discussed.

Separate extracts, containing a relatively heat-stable (luciferin) and a heat-labile (luciferase) component, which react with the emission of visible light in vitro (the luciferin-luciferase reaction), have been obtained from not quite a dozen types of the multitude of luminescent organisms known. They have been obtained thus far from certain bivalves [*Pholas* (*1*), *Rocellaria* (*2*)], elaterid [*Pyrophorus* (*3*)] and lampyrid [*Photinus*, *Photuris*, *Luciola* (*4*, *5*)] fireflies, ostracod crustacea [*Cypridina* (*6*), *Pyrocypris* (*7*)], a marine polychaete worm [*Odontosyllis* (*7*)], decapod shrimps [*Systallaspis* (*8*) *Heterocarpus* (*9*)], a fresh water limpet [*Latia* (*10*)], luminous bacteria [*Achromobacter*, *Photobacterium* (*11*)], a protozoan [*Gonyaulax* (*12*)], fish [*Parapriacanthus* (*13*), *Apogon* (*14*)], luminous fungi [*Collybia*, *Armillaria* (*15*)], and a pennatulid [*Renilla* (*16*)]. Numerous tests, over the past several decades, for light emission in cross reactions between components of different luminous organisms have indicated that the luciferins and luciferases are specific for a given type, definite cross reactions being rarely found (*17*), and then only with extracts of organisms rather closely related, such as two genera of ostracods (*7*) or two families of fireflies (*4*, *18*). Moreover, among the three biochemically best known systems, namely, those of *Cypridina*, the firefly, and luminous bacteria, the diffusible factors are all chemically different and noninterchangeable, except for oxygen, which is required by each (*17*).

These facts have led to the view that, in general, the luciferin and luciferase of one type of luminescent organism are quite different from those of another type (*17*, *19*), a view that accords not only with profound differences in emission spectra (*17*, *20*) but also with the utterly random occurrence of bioluminescence on the phylogenetic scale of animals from protozoa to fishes as well as among bacteria and higher fungi. Experiments reported in this paper (*21*), however, favor a modification of this view, according to the following evidence.

Qualitative observations in regard to the recently discovered example of a luciferin-luciferase reaction in crude extracts of the photogenic organs of a fish, *Parapriacanthus* (*13*), indicated a reciprocal cross reaction with the luciferin and luciferase in crude extracts from a second luminescent fish, *Apogon* (*14*), representing a different family. The *Apogon* system was then found to exhibit an astonishing cross reaction with that of the crustacean, *Cypridina*. No visible evidence, however, was found of a cross reaction between the *Parapriacanthus* and *Cypridina* systems, nor between *Apogon* and the Japanese firefly *Luciola*, or the clam *Pholas*. Since purely visual observations with crude extracts have occasionally proved misleading, quantitative studies and experiments with some highly purified extracts of *Apogon* and *Cypridina* have been carried out, with convincing results.

Quantitative data of cross reactions in the *Apogon* and *Cypridina* systems are illustrated in Fig. 1. *Apogon* has one anterior and two posterior photogenic organs whose luciferin and lucif-

erase react with each other as well as with those of *Cypridina*, though not with components extracted from non-photogenic tissues of the fish. With a given enzyme concentration, the total light emitted is proportional to the amount of luciferin added. With a given concentration of luciferin, the rate of decay of luminescence is proportional to the enzyme concentration, except when the luciferin is in great excess, as it appears to be in the *Apogon* organ. Thus, when either a freshly excised or desiccated organ (the latter weighing about 1 mg) is ground and suspended in 1 or 2 ml of water, luminescence may persist for hours. If greatly diluted *Cypridina* luciferase is added early to this reaction mixture, a bright luminescence of short duration occurs. Evidently, *Apogon* luciferase is either less active than that of *Cypridina* toward the luciferin of either organism, or else it is present in only minute amounts. Efforts to concentrate it have been only partially successful, because of the presence of inhibitors which become simultaneously more concentrated. A relatively low activity of luciferase along with an excess of luciferin, resulting in a long-lasting, relatively dim luminescence, is possibly of survival value to the fish.

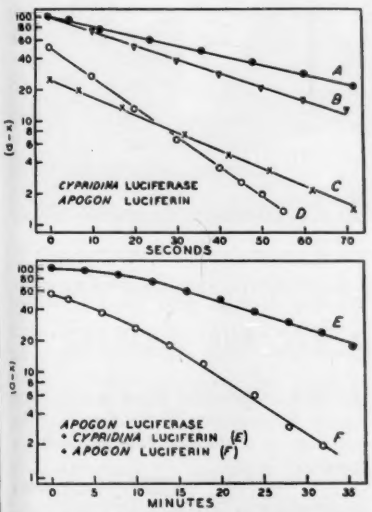

Fig. 1 Luminescence in cross reactions between the luciferin and luciferase of *Apogon* and *Cypridina* in phosphate-buffered solutions at neutral *p*H and room temperature. The data are plotted in the manner of a reaction first order with respect to the amount of luciferin, which is taken proportional to the total light, *a*, measured in arbitrary units by a photomultiplier integrating device, *x*, representing the fraction of the total light emitted up to a given time as indicated on the abscissa. Reaction mixtures were as follows: (*A*), *Cypridina* luciferase + anterior *Apogon* luciferin; (*B*), *Cypridina* luciferase, concentration 1, + posterior *Apogon* luciferin, concentration 2, *Apogon* luciferin, concentration 1; (*C*), *Cypridina* luciferase, concentration 1, + posterior *Apogon* luciferin, concentration ¼; (*D*), *Cypridina* luciferase, concentration ½; (*E*), anterior *Apogon* luciferase + *Cypridina* luciferin; (*F*), anterior *Apogon* luciferase + anterior *Apogon* luciferin. Differences in total light and slopes of only curves (*B*), (*C*), and (*D*) are significant.

The photogenic organs of *Apogon* communicate with the gut, but are histologically and morphologically distinct (*22*). Careful examination of the stomach and intestinal contents of over 40 specimens have revealed no remains of small crustacea, such as *Cypridina* or related organisms. (These would not be expected from the habitat of the fish at a depth of 40 fathoms.) Thus the cross reactions cannot reasonably be attributed to the presence of extraneous, crustacean material such as was once responsible for the seeming luminescence of a flying fish (*23*).

The purification and properties of the *Apogon* system have been investigated and will be reported in detail elsewhere. It is worth noting here, however, that none of the diffusible factors known to function in other systems (*17*) have been found to increase the light of the *Apogon* system (*24*). The absorption spectra of chromatographed luciferins of *Apogon* and *Cypridina* have points in common, but are not identical, and the changes on oxidation are not the same. Both luciferins are associated with a yellow color by ordinary light and yellow fluorescence by ultraviolet light. The two luciferins are qualitatively alike in behavior during Anderson's (*25*) purification procedure, in undergoing an initially reversible, dark oxidation by oxygen or ferricyanide (*26*), in varying sensitivity to cyanide (*27*) at corresponding stages of purification, and in having nearly the same Rf on paper with Tsuji's (*28*) solvent. Under the conditions employed, the optimum *p*H for total light is associated with the substrate, being close to 7.0 when either enzyme is used with *Cypridina* luciferin, and between 6.3 and 6.5 with *Apogon* luciferin, while the optimum *p*H for rate of light emission is correlated with the specific enzyme, being from 7.6 to 7.8 with *Cypridina* luciferase, and about 7.2 with *Apogon* luciferase, using either of the two luciferins.

The enzymes differ in that *Cypridina* luciferase is sensitive to $4 \times 10^{-4}M$ versene, whereas *Apogon* luciferase is not, and in that *Cypridina* luciferase withstands exhaustive dialysis, whereas *Apogon* luciferase progressively loses activity in a manner that can be initially restored by the addition of dialyzate.

Although the similarities in the luminescent systems of a fish and crustacean could represent a rare, evolutionary coincidence, they as likely indicate that more of a thread of unity exists in the comparative biochemistry of luminescence among diverse types of organisms than has been hitherto supposed.

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21. This work has been aided in part by National Science Foundation grants G 4578 and G 6106, by Office of Naval Research contract Nonr 1353(00), by the Eugene Higgins Fund allocated to Princeton University, and by the Japanese National Commission for UNESCO. Reproduction, translation, publication, use, and disposal in whole or in part by or for the United States Government is permitted.

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2 March 1960

Experimental Shark Pens at the Lerner Marine Laboratory

Abstract. A new facility has recently been added at the Lerner Marine Laboratory, Bimini, Bahamas, which makes it possible for the first time to work experimentally with large elasmobranchs up to 15 feet in length.

The Lerner Marine Laboratory, generous gift of Michael Lerner and now in its 12th year, is operated by the American Museum of Natural History in New York as one of the Museum's five field stations. Situated on the east side of the Gulf Stream on the island of Bimini, only 53 air miles due east of Miami, the laboratory has become an important center for the study of semitropical marine fauna and flora of the Caribbean area. With the addition of three shark pens, constructed with funds provided by the Office of Naval Research, investigators may now study several species of large elasmobranchs (*1*) under excellent conditions for observation and experiment.

Two of the pens (holding and observations pens) measure approximately 40 by 80 feet and are 7 feet deep at mean high tide (Fig. 1). Between the two large pens and connected with them by gallows gates, is a smaller operating pen 16 by 40 by 7 feet. All three pens are enclosed in chain-link fencing made of stainless steel 11-gauge wire stapled to pressure-treated wood pilings. A dock 10 feet wide surrounds the three pens and a 2-foot wide catwalk extends along the north and south side of the central operating pen. Over the operating pen is a monorail carrying an electrically driven 2-ton hoist and above this is a viewing platform 15 by 20 feet. Since the water is of unusual clarity at Bimini, it is possible to observe in considerable detail, from the platform, the behavior of normal and experimental sharks as they swim over the white coral sand bottom of the observation and holding pens.

When a shark in the holding pen is selected for experiment it is separated from the others by a net having a 4-inch mesh, and guided through the gallows gate into the operating pen, where it is confined by stout netting spread over rectangular metal frames. By means of the electric hoist the netting on the floor and side of the operating pen may be elevated, further confining the shark and raising it to the water's surface

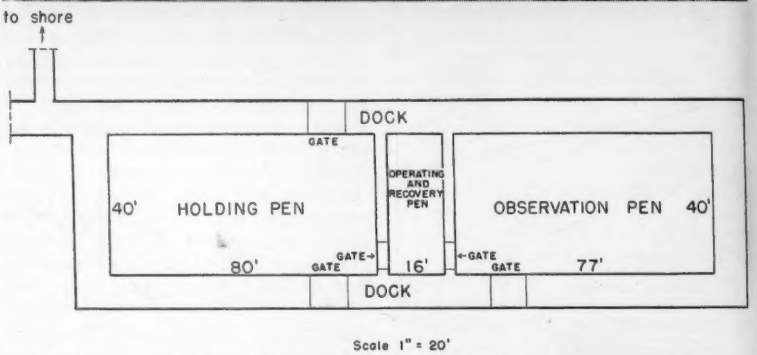

Fig. 1. (Top) View of the shark pens at the Lerner Marine Laboratory, looking north. [Photograph by Langley Wood]. (Bottom) Diagram of shark pens showing relative sizes and positions of the pens.

alongside the dock. In this position the shark is anesthetized by spraying MS222 into its mouth and over its gills with a pump-type hand sprayer attached to a 4-foot brass delivery tube (*2*). Ordinarily 1 liter of MS222 (1:1000 dilution) can be delivered in 6 to 10 seconds into the mouth of the shark. It takes only 60 to 90 seconds for the MS222 to anesthetize a 400- to 500-lb. shark, after which it may be raised to the dock on a canvas-strap stretcher and operated on with safety for as long as 20 minutes. After the operation the shark is allowed to recover in the operating pen and then is directed through the gallows gate into the observation pen for subsequent study. In addition to experiments on the visual and olfactory organs now in progress, sharks are currently being conditioned for an investigation of hearing and the influence of various sounds on behavior patterns.

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HENRY KRITZLER
Lerner Marine Laboratory,
DuPont Building, Miami, Florida

References and Notes

1. Species readily available include: lemon shark (*Negaprion brevirostris*), dusky shark (*Carcharhinus obscurus*), brown shark (*Carcharhinus milberti*), bull shark (*Carcharhinus leucas*), reef shark (*Carcharhinus falciformis*), tiger shark (*Galeocerdo cuvier*), and nurse shark (*Ginglymostoma cirratum*).
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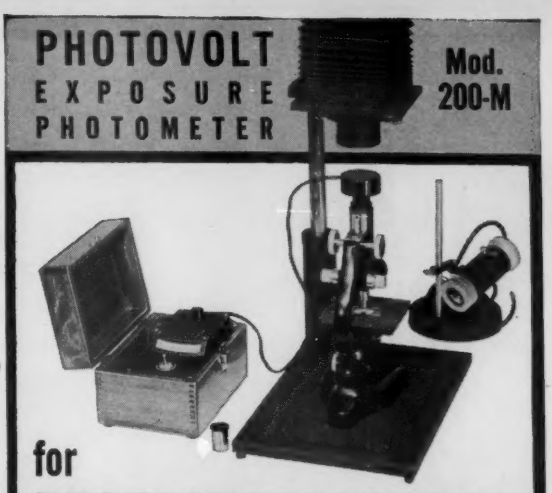

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Meetings

Congress of Anatomists

The 7th International Congress of Anatomists and the 73rd annual meeting of the American Association of Anatomists were held jointly in New York City, 11–16 April. All of the scientific sessions, except for certain symposia, were held at the Statler Hilton Hotel. The Albert Einstein College of Medicine, the College of Physicians and Surgeons of Columbia University, Cornell University Medical College, New York Medical College, New York University–Bellevue Medical Center, New York University College of Dentistry, the Downstate Medical Center of the State University of New York, and the Rockefeller Institute for Medical Research were the host institutions. Members of the congress committee were George W. Corner (president of the congress), H. Stanley Bennett (president of the American Association of Anatomists), Joseph C. Hinsey, Don W. Fawcett, and Wilfred A. Copenhaver. The local committee consisted of Ernst Scharrer, James B. Hamilton, Leonard L. Ross, and Roy C. Swan; the program committee, of Oliver P. Jones, Murray L. Barr, Edward H. Bloch, Edward W. Dempsey, Herbert Elftman, William U. Gardner, and Pinckney J. Harman.

The attendance (exclusive of guests) approximated 1700 individuals, including 337 from outside the United States. These latter came from 41 countries (from Canada, 76; Great Britain, 65; Germany, 24; Japan, 21; France and Mexico, 17 each; the U.S.S.R., 14; the Netherlands, 12; Argentina and Italy, 8 each; Sweden, 7; China, India, and Yugoslavia, 5 each; Australia, Belgium, and Switzerland, 4 each; Brazil, Ecuador, El Salvador, Finland, North Ireland, and the Union of South Africa, 3 each; Austria, Colombia, Peru, Thailand, and Venezuela, 2 each; the Belgian Congo, Costa Rica, Denmark, Egypt, Hungary, Korea, New Zealand, Norway, Panama, the Philippines, Spain, Uganda, and Vietnam, 1 each). A grant from the U.S. Public Health Service made it possible for many of the foreign participants to attend; 159 individuals from overseas were aided by this grant.

Six hundred and two papers, 36 motion pictures, and 126 demonstrations were presented. The papers given at the regular sessions may be grouped, for convenience, into the following categories: Neurology (general, experimental, comparative), 134; microscopic anatomy (histology, cytology, histochemistry), 123; embryology (human, comparative, and experimental embryology; teratology; reproduction),

91; general morphology (gross anatomy, physical anthropology, comparative anatomy), 72; endocrinology, 71; hematology, 32; histophysiology, 11; histopathology, 11. In addition, there were five symposia: on the eye (33 papers), on anatomy and biomechanics of bone (8 papers), on pituitary circulation (4 papers), on bone-marrow circulation (3 papers), and in tribute to Elmer G. Butler by his former students (9 papers).

Entertainment provided for congress members and guests included a smoker, a banquet, a boat trip around Manhattan, and an evening reception at the Metropolitan Museum of Art.

WILLIAM L. STRAUS, JR.
Johns Hopkins University, Baltimore, Maryland

Forthcoming Events

September

3–10. International Cong. of Preventive Medicine and Social Hygiene, 8th, Bad Aussee, Austria. (A. Rottmann, Liechtensteinstrasse 32/4, Vienna 9, Austria)

4–9. Cell Biology, 10th intern cong., Paris, France. (M. Chèvremont, Institut d'Histologie, 20, rue de Pitteurs, Liege, Belgium)

4–9. Laurentian Hormone Conf., Mont Tremblant, Quebec, Canada. (Arrangements Committee, Laurentian Hormone Conf., 222 Maple St., Shrewsbury, Mass.)

4–10. International Soc. of Orthopaedic Surgery and Traumatology, 8th cong., New York, N.Y. (A. Bailleux, Société de Chirurgie Orthopedique et de Traumatologie, 34, rue Montoyer, Brussels, Belgium)

4–10. World Cong. of Anaesthesiologists, Toronto, Canada. (R. A. Gordon, 516 Medical Arts Bldg., Toronto 5)

4–14. International Societies of Hematology and Blood Transfusion, 8th cong., Tokyo, Japan. (S. Murakami, Blood Transfusion Laboratory, Japanese Red Cross Soc., Shibuya, Tokyo)

5–7. Society for Biological Rhythm, 7th conf., Siena, Italy. (A. Sollberger, Dept. of Anatomy, Caroline Inst., Stockholm 60)

5–8. Legal and Administrative Problems of Protection in the Field of the Peaceful Applications of Atomic Energy, intern. symp., Brussels, Belgium. (Communauté Européenne de l'Energie Atomique, rue Belliard 51–53, Brussels)

5–9. Chemical Engineering (Czechoslovak Chemical Soc.), Prague, Czechoslovakia. (Technická 1905, Prague-Dejvice, Czechoslovakia)

5–10. Microbiology of Non-Alcholic Beverages, 5th intern. symp., Evian, France. (D. A. A. Mossell, Intern. Assoc. of Microbiological Societies, c/o Central Inst. for Nutrition Research, Catherinjnesingel 61, Utrecht, Netherlands)

5–9. Medium and Small Power Reactors, conf., Vienna, Austria. (International Atomic Energy Agency, 11 Kärntner Ring, Vienna 1)

5–10. Operational Research, 2nd intern. conf., Aix-en-Provence, France. (International Federation of Operational Research Societies, 11 Park Lane, London, W.1)

5–12. International Soc. of Bioclimatology and Biometerology, 2nd cong., London, England. (E. M. Glaser, Dept. of Physiology, London Hospital Medical College, Turner St., London, E.1)

5–15. International Scientific Radio Union, London, England. (R. L. Smith-Rose, Radio Research Station, DSIR, Ditton Park, Slough, Bucks, England)

5–17. Photogrammetry, 9th intern. cong., London, England. (J. B. P. Angwin, Intern. Soc. for Photogrammetry, 18 Cavendish Sq., London, W.1)

6–7. Some Fundamental Aspects of Atomic Reactions, symp., Montreal, Canada. (J. C. Polanyi, Dept. of Chemistry, Univ. of Toronto, Toronto 5, Canada)

6–8. Nuclear and Radio-Chemistry, symp., Chalk River, Ontario, Canada. (R. H. Betts, Atomic Energy of Canada Ltd., Chalk River, Ontario)

6–8. Society of General Physiologists, annual, Woods Hole, Mass. (J. W. Green, Rutgers Univ., New Brunswick, N.J.)

6–17. Use of Radioactive Isotopes in the Physical Sciences and Industry, conf., Copenhagen, Denmark. (International Atomic Energy Agency, 11 Kärntner Ring, Vienna 1, Austria)

7–8. Canadian Textile Seminar, 7th, Kingston, Ontario. (J. M. Merriman, Textile Technical Federation of Canada, 223 Victoria Ave., Westmount, P.Q.)

7–9. Canadian High Polymer Forum, 10th, Ste. Marguerite, near Montreal, Quebec, Canada. (D. A. I. Goring, CHPF, Pulp and Paper Research Inst., McGill Univ., Montreal)

7–9. International Soc. of Geographical Pathology, 7th conf., London, England. (J. S. Young, ISGP, c/o Dept. of Pathology, Forresterhill, Aberdeen, Scotland)

7–9. International Union of Pure and Applied Physics, Ottawa, Canada. (P. Fleury, 3, Boulevard Pasteur, Paris 15°, France)

7–9. Joint Automatic Control Conf., Boston, Mass. (H. A. Miller, Taylor Instrument Co., 95 Ames St., Rochester 1, N.Y.)

7–10. Calorimetry, 15th conf., Gatlinburg, Tenn. (D. W. Osborne, Argonne Natl. Laboratory, P.O. Box 299, Lemont, Ill.)

8–9. Technical Comunications, 2nd annual, Dayton, Ohio. (D. G. Peterson, Jr., Soc. of Technical Writers and Editors, 4564 Marlin Ave., Dayton 16, Ohio)

8–10. American Political Science Assoc., New York, N.Y. (E. M. Kirkpatrick, 1726 Massachusetts Ave., NW, Washington 6)

8–10. Great Issues of Conscience in Modern Medicine, Hanover, N.H. (G. O'Connell, Dartmouth College News Service, Hanover)

8–10. Parapsychological Assoc., 3rd. annual, New York, N.Y. (W. G. Roll, Parapsychology Laboratory, Duke Univ., Durham, N.C.)

8–18. History of Medicine, 17th intern. cong., Athens and Isle of Cos, Greece. (S. Oeconomos. Faculty of eMdicine, National and Capodistrian Univ. of Athens, Odos panepistimiou, Athens, Greece)

10–11. Air Pollution, intern. cong., New York, N.Y. (A. B. Conlin, Jr., ASME, 29 W. 39th St., New York 18)

11–15. International College of Surgeons, 12th intern. cong., New York, N.Y. (M. Thorek, ICS, 850 W. Irving Park Rd., Chicago 13, Ill.)

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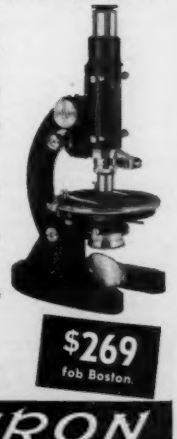

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11–16. American Chemical Soc., 138th annual, New York, N.Y. (A. T. Winstead, ACS, 1155 16th St., NW, Washington 6)

11–16. Illuminating Engineering Soc., natl. technical conf., Pittsburgh, Pa. (A. D. Hinkley, IES, 1860 Broadway, New York 23)

12–13. International Conf. on Trichinellosis, Warsaw, Poland. (Z. Kozar, Polish Parasitological Soc., Zaklad Parazytologii, PAN, Warszawa, Pasteura 3, Poland)

12–14. Entomological Soc. of Canada—Entomological Soc. of Saskatchewan, annual joint meetings, Saskatoon, Sask., Canada. (L. L. Reed, ESC, K. W. Neatby Bldg., Carling Ave., Ottawa, Canada)

12–15. Atomic Masses, intern. conf., Hamilton, Ontario, Canada. (H. E. Duckworth, Dept. of Physics, McMaster Univ., Hamilton)

12–15. Society of Automotive Engineers, Milwaukee, Wis. (R. W. Crory, SAE, Meetings Operation Dept., 485 Lexington Ave., New York 17)

12–16. International Council of the Aeronautical Sciences, 2nd intern. cong., Zurich, Switzerland. (J. B. Bidwell, Inst. of the Aeronautical Sciences, 2 E. 64 St., New York 21)

12–17. World Federation of Occupational Therapists, Sydney, Australia. (Liverpool School of Occupational Therapy, Victoria Rd., Huyton, Liverpool, England)

13–14. Bionics, symp., Dayton, Ohio. [Commander, Wright Air Development Division, Attention: WWRDA (Maj. J. E. Steele, Wright-Patterson Air Force Base, Ohio)]

13–15. Instruments and Measurements, 5th intern. conf., Stockholm, Sweden. (Tekn. Lic. Helge von Koch, Kungl. Tekniska Högskolan, Stockholm 70)

14–15. Aspects of Internal Irradiation of Mammals, Saratoga, Wyo. (T. F. Dougherty, Univ. of Utah, Salt Lake City)

14–16. Tube Techniques, 5th natl. conf., New York, N.Y. (D. Slater, College of Engineering. Research Div., New York Univ., 346 Broadway, New York 13)

15–16. Engineering Management Conf., 8th annual, Chicago, Ill. (E. O. Kirkendall, AIME, 29 W. 39 St., New York 18)

15–17. Radio Soc. of Great Britain, natl. convention, Cambridge, England. (Secretary, RSGB Convention Committee, 37 Metcalfe Rd., Cambridge, England)

16–18. Cori's Ester and Phosphorylated Glucides, 1st intern. symp., Milan, Italy. (Segreteria Organizzativa del 1st Symposium Internazionale sull'estere di Cori e sui glucidi fosforilati, Via Modica 6, Milan)

16–21. European Cong. on Infantile Neuro-Psychiatry, Paris, France. (G. Belaubre, 14 rue Drouot, Paris)

16–22. World Medical Assoc., Berlin, Germany. (General Secretary, WMA, 10, Columbus Circle, New York 19)

18–21. Forensic Pathology, 2nd intern., New York, N.Y. (C. Larsen, Tacoma General Hospital, Tacoma 5, Wash.)

18–25. Inter-European Cong. of Cardiology, 3rd, Rome, Italy. (V. Puddu, Clinica, Medica, Università-Policlinico, Rome)

19–21. Space Electronics and Telemetry, 5th natl. symp., Washington, D.C. (H. W. Royce, Glenn L. Martin Co., Mail Stop H–2035, Baltimore 3, Md.)

(See issue of 29 July for comprehensive list)

New Products

The information reported here is obtained from manufacturers and from other sources considered to be reliable. Neither Science *nor the writer assumes responsibility for the accuracy of the information. All inquiries concerning items listed should be addressed to the manufacturer. Include the department number in your inquiry.*

■ ANGULAR MOTION TRANSDUCER is sensitive to a rotation of 0.012 deg and has a range of ±3 to ±5 deg. The magnified input of an angular motion actuates a wiper that moves across a potentiometer producing a d-c output signal. The unit is completely sealed. Flat frequency response to 30 cy/sec is said to have been observed. Life expectancy is said to be over 100,000 cycles. (Bourns Inc., Dept. Sci683, P.O. Box 2112, Riverside, Calif.)

■ BURETTE is made from precision bore tubing with a free-riding hollow glass piston accurately fitted to the bore. According to the manufacturer, the burette is capable of accuracy better than ±0.02 percent. The free piston system provides automatic zero when filling. A movable rod in the top assembly permits adjustment of the zero point. The scale, graduated in 0.1-ml subdivisions, is linear, permitting use of a vernier for reading. (Kern Laboratory Supply Co., Dept. Sci690, 2611 Exposition Blvd., Los Angeles, Calif.)

■ VOLTAGE REFERENCE is said to be equivalent in accuracy to the best unsaturated standard cells. While the source is a zero current drain instrument it can be operated into any impedance or short circuited without damage. Power input is 90 to 135 volts, 60 cy/sec, 25 v-amp. Output is 1.0000 and 1.0185 volts. Accuracy is said to be ±0.01 percent of nominal with certification of actual output to ±0.001 percent. Temperature range for this accuracy is 15° to 35°C. (Sensitive Research Instrument Corp., Dept. Sci693, 310 Main St., New Rochelle, N.Y.)

■ DUAL-GUN STORAGE TUBE is said to have a minimum resolution of 1200 lines per diameter. Stored signals can be held for many hours, read several thousand times, or erased in a fraction of a second. Decay of the display can be controlled at half-life times between 3 sec and 10 min. (Raytheon Co., Dept. Sci694, 55 Chapel St., Newton, Mass.)

■ BUBBLE DETECTOR operates by passing a pulsed ultrasonic signal through the liquid under observation which is flowing between transmitter and receiver heads. The presence of bubbles weakens or interrupts the signal. Individual bubbles are detected and the strength of the output signal varies with both size and number of bubbles. Minimum detectable bubble size is 0.001 in. in diameter. Output is 0 to 4 ma into 2500 ohms. Response time is less than 0.1 sec for 63-percent response. (Taylor Instrument Co., Dept. Sci695, 95 Ames St., Rochester 1, N.Y.)

■ FLAME PHOTOMETER monitors chloride ion in boiler feed water by measuring sodium normally associated with the chloride ions. Normal range is 0.1 to 10 parts per thousand million. The instrument consists of an optical unit and a control panel. The optical assembly houses a sample manifold, burner, monochromator, and multiplier phototube; the control panel houses a high-voltage supply, amplifier, automatic standardizing system, sample sequence selector, and recorder. The sample manifold may accommodate up to five sample streams. (Waters Associates, Dept. Sci692, 45 Franklin St., Framingham, Mass.)

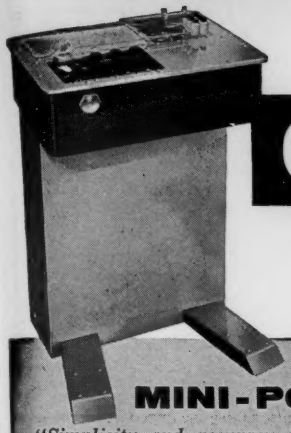
■ RECORDER of strip-chart, self-balancing, potentiometer type is said to be accurate within ±0.25 percent of full scale or ±20 μv, whichever is greater, without necessity for standardization. A single mercury cell serves as reference voltage. Chart width is 250 mm with 1-second full-scale pen travel. Standard range is 125 mv. Seven other ranges from 1.0 to 50 mv are interchangeable by means of plug-in resistors. Standard chart speed is 1 in./min with eight speeds from 1/60 to 12 in./min available by interchange of motor. Zero displacement is adjustable over full chart width. (E. H. Sargent & Co., Dept. Sci-702, 4647 W. Foster, Chicago 30, Ill.)

■ CAPACITANCE METER is a transistorized instrument that displays capacitance and dissipation factor digitally. Two test frequencies are provided, 120 and 1000 cy/sec. Four ranges at each frequency cover capacitance from 10 pf to 999.9 μf and dissipation factor from 0.001 to 0.999. Average measurement time is 1 second. Output is provided in the form of decimal contact closures as well as visually. (Electro Instruments, Inc., Dept. Sci698, 3540 Aero Court, San Diego 11, Calif.)

■ LABORATORY HOT PLATE with 3-in. diameter top incorporates a thermostatic control unit that provides stepless selection of temperature from 6°C above ambient to 370°C. A built-in anticipatory sensing device is said to assure negligible overshoot in initial heatup, with temperature thereafter constant within ±3°C. Temperature of 370°C is reached in 9 minutes from cold start. (Thermo Electric Manufacturing Co., Dept. Sci703, 568 Huff St., Dubuque, Iowa)

JOSHUA STERN

National Bureau of Standards, Washington, D.C.



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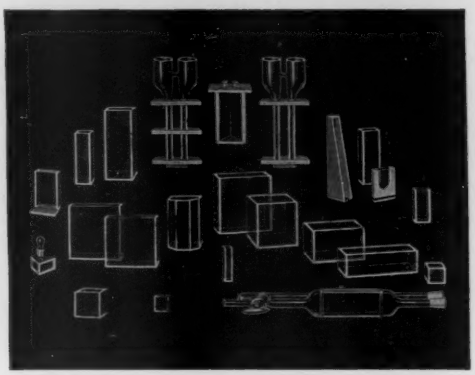

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Microbiologist, Ph.D. 11 years' experience in research, teaching medical-dental students, clinical laboratories. Available for teaching and/or research. Box 169, SCIENCE. X

POSITIONS WANTED

Biochemist, Ph.D. 7 years' diversified industrial experience in fermentation, enzyme applications in foods, brewing, pharmaceuticals, and so forth, and enzyme characterization and purification. Responsible administrative or supervisory position only. Will relocate foreign or domestic. Box 168, SCIENCE. X

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Box 8299 Philadelphia 1, Pa.

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1937 West Main Street
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(a) **Microbiologist,** teaching, research at undergraduate, graduate levels; $7000; Canada. (b) **Biochemist,** B.S., M.S., experienced clinical chemistry to head department, 300-bed voluntary general hospital; to $8000; midwestern college city 75,000. (c) **Bacteriologist,** B.S., 2 years' hospital experience; 275-bed general hospital; to $5400; Texas. (d) **Biochemist,** Ph.D., experienced hospital chemistry to head department, large California hospital; $10,000. (e) **Microbiologist,** Ph.D. familiar single, suspended, monolayer cell culture techniques to head cell production facility, conduct research; prominent pharmaceutical company; Midwest. (f) **Biochemist,** interested molecular biology for research in studies of nucleic acid metabolism, related areas; to $9000; prominent southwestern university research institute. (g) **Bacteriologist,** B.S., M.S., to supervise department, 350-bed California hospital; to $6200. (Please write for an analysis form. Our 64th year: Founders of the counselling service to the medical profession; serving medicine with distinction over half a century.) Science Division, Woodward Medical Bureau, Ann Woodward, Director, 185 North Wabash, Chicago. X

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